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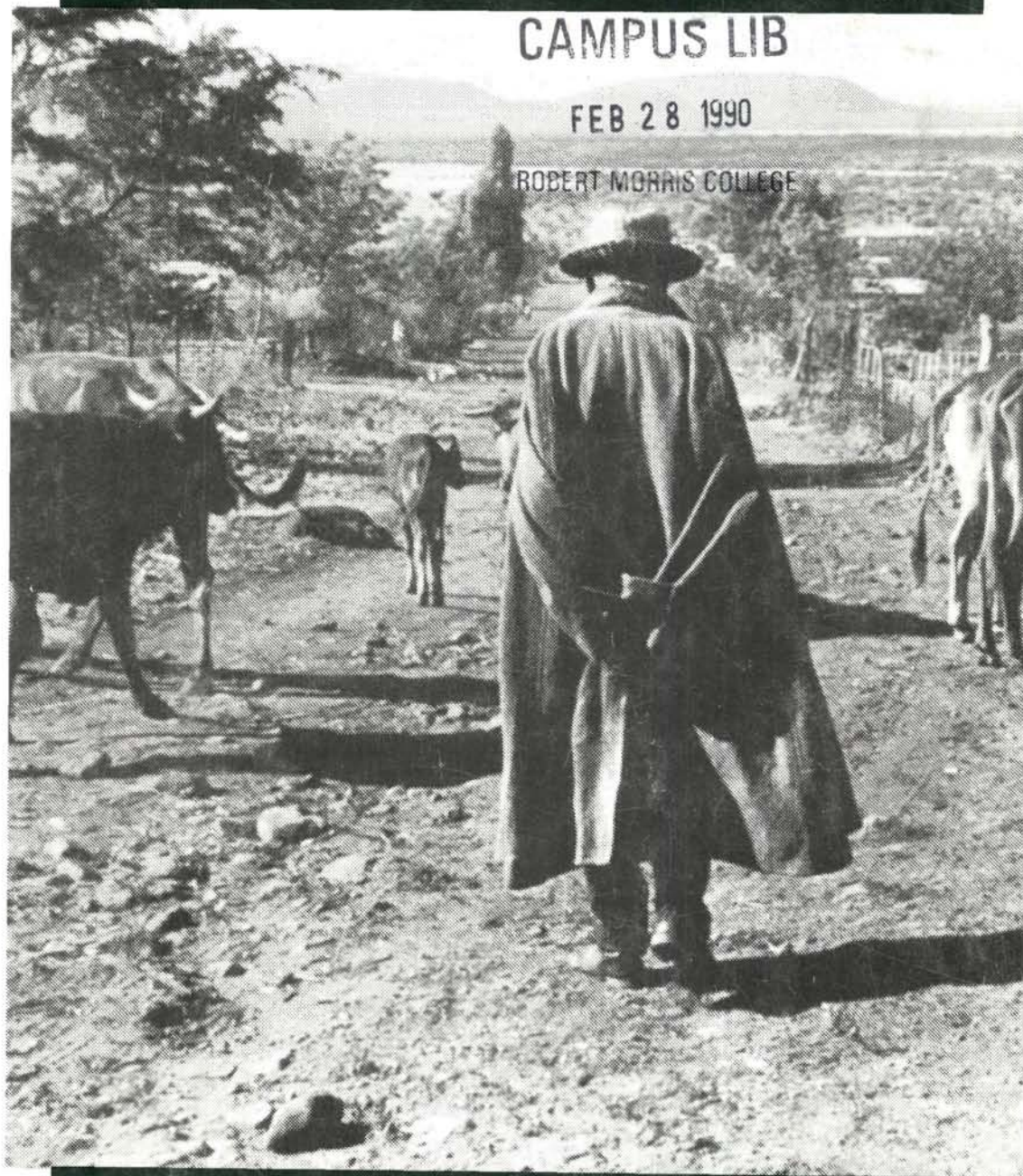
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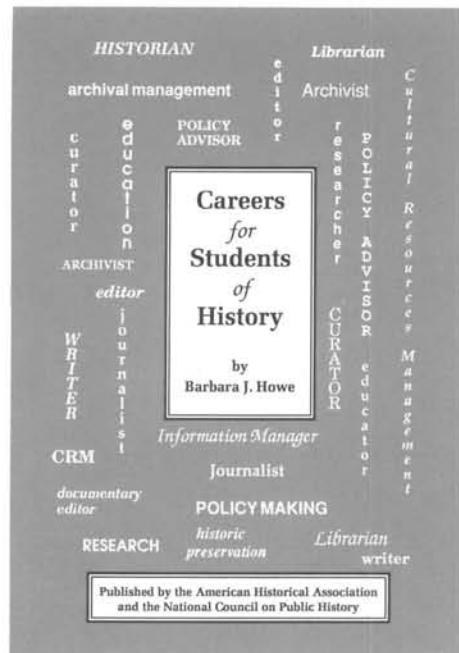
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Cover illustration: A detail from a photograph of Kas Maine, a sharecropper born in the South-Western Transvaal, Republic of South Africa. This picture shows Maine at age ninety in what was then the small rural town of Ledig (photographer, David Goldblatt, 1983). See the article in this issue by Charles van Onselen, "Race and Class in the South African Countryside: Cultural Osmosis and Social Relations in the Sharecropping Economy of the South-Western Transvaal, 1900–1950," pp. 99–123.

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In This Issue

In his presidential address, **Louis R. Harlan** calls upon the members of the American Historical Association to respond to the changing needs of the profession. Toward that end, he proposes a membership campaign, strengthened ties with non-academic historians, more attention to the needs and concerns of social studies teachers, expansion of the Association's advocacy efforts, and a capital fund campaign in support of special initiatives.

For much of his scholarly life, Louis Harlan has explored questions related to the history of race in the United States. In this issue, we look anew at that history, in part by placing it in the broader context of Western Europe and South Africa. **Jane Landers** uses parish records and a wide range of other sources to recapture the elusive history of slaves who fled the Carolinas to find freedom in Spanish Florida in the early eighteenth century. She describes their adaptation to the new Hispanic environment, as well as the uses Spaniards made of these refugees for political and military ends.

Building on the work of Carter Woodson, John Hope Franklin and Ira Berlin, **Loren Schwener** analyzes changes in the size, wealth, and social values of black economic elites of the Upper and Lower South between 1790 and 1880. Census returns, tax and probate records, newspapers, and manuscript collections reveal some surprising differences between black property owners before and after the Civil War. Very different sorts of material figure in **Kenneth S. Greenberg's** study of white male honor in the antebellum South. When P. T. Barnum brought the Fiji Mermaid to the South, arguments about its appearance escalated into a conflict that stopped just short of a duel. Greenberg uses incidents of this kind to demonstrate the concern of Southern men of honor for the surface of things, for projections rather than underlying realities. He also suggests ways in which an action can be read as a text in a "language" of honor.

The nineteenth century was an age of emancipation and national unification throughout much of the Western world. Yet, argues **Steven Hahn**, while Prussian Junkers and Brazilian *fazendeiros* were able to forge powerful postemancipation alliances between industry and large-scale agriculture, Southern planters in the United States lost not only their slaves but almost all economic and political influence, especially at the national level. Hahn's essay thus shows the benefits of a comparative perspective on the history of race and class. **Charles van Onselen's** research does the same—but in this case with the techniques of oral history. Van Onselen uses oral testimony to investigate the interaction of race and class in twentieth-century South Africa. He finds that, in the South-Western Transvaal, relations between white landlords and black tenants, as expressed in work, recreation, health, religious life, and the search for justice, were mediated and moderated by the structure of the local sharecropping economy.

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memoir of naval service in World War II. His presidential address developed out of his involvement as co-chair (with Richard H. Kohn) of the AHA's Ad Hoc Committee on the Future of the American Historical Association, and he reports that it owes much to stimulating association with other committee members and with James B. Gardner, Deputy Executive Director of the AHA and staff liaison to the committee.

Jane Landers is a visiting assistant professor in the department of history at the University of Florida and director of the History Teaching Alliance, a national program of teaching collaboratives. She received her doctorate from the University of Florida and specializes in the history of blacks in colonial Latin America and the southeastern United States. She is the historical consultant on the Ft. Mose Archaeological Project funded by the state of Florida and has helped prepare a traveling museum exhibit concerning the site, as well as a video and teaching materials for pre-collegiate teachers. She is currently revising her dissertation, “Black Society in Spanish St. Augustine, 1784–1821,” and is beginning a comparative study of free black communities in colonial Latin America. An earlier version of the article in this issue won the President's Prize of the Florida Historical Society.

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editor of *From Tennessee Slave to St. Louis Entrepreneur: The Autobiography of James Thomas* (1984). His study *Black Property Owners in the South, 1790–1915* is in press (Illinois) and will be forthcoming this year. The recipient of a Senior Fellowship for College Teachers from the National Endowment for the Humanities, he is currently working on an editorial project involving state and local petitions concerning race, slavery, and free blacks in the nineteenth-century South.

The Future of the American Historical Association

LOUIS R. HARLAN

I ASK ALL MEMBERS OF THE AMERICAN HISTORICAL ASSOCIATION tonight and in the days ahead to give serious thought to the needs of the historical profession in the 1990s and to how well the AHA, as the umbrella organization for all historians in the United States, fulfills its obligation to lead the profession. How well does it serve its broad and changing constituency, and how can it command the loyalty, interest, and participation of the entire historical profession? How can members, as well as elected leaders, help to strengthen the AHA and the historical profession? These questions do not lend themselves to easy answers. In part, the answers depend on what happens in the near future, and historians have no special license to predict or determine the future. Nevertheless, I propose to open the discussion by suggesting some changes in the way we conduct our business to meet the shifting needs of our profession. The changes I shall propose are not so much structural as functional and attitudinal.

The greatest risk the AHA faces, it seems to me, is that involved in drifting and dawdling at the usual pace of a learned society. Such a course risks the danger that an increasingly specialized and compartmentalized profession will pass by the AHA and declare it obsolete. I raise this specter not out of any spirit of disloyalty. I have been a member for forty years, and you have just honored me with the office of president. I simply want to convey to you a sense of urgency. I could speak all night on the good works of the AHA, past and present, and I hope to do so on some future occasion. Tonight, however, I want to disturb the after-dinner nap that perhaps you thought came with the price of admission. My theme will be the ways in which the AHA is falling short of its potential and how it can correct its course by certain changes. If my judgment seems fallible or you disagree with my assessment of the needs of the historical profession, at least I shall stimulate debate on the direction and future of the AHA.

Let us first consider the present state of the history profession, as a prolegomenon to a discussion of how the AHA might meet the profession's needs. There is considerable recent evidence that the historical profession, and academia in general, is climbing out of a depression that began with the job market crash of the early 1970s. In that period, we had a large pool of unemployed young historians, newly minted Ph.D.s, many of whom became discouraged by the competition for ever fewer positions and left for other occupations. At the same time, history departments shrank in size, and an aging faculty became tenured in. The pathology spread to other aspects of the profession. Our most creative scholars found that productivity did not create mobility; the discontented could not move; doctoral training programs were demoralized by the difficulty or even the impossibility of placing their doctoral students. Perhaps they shared the ebbing faith in their

discipline, for historians generally stood by as many institutions removed history courses from core requirements. During this same period, the AHA underwent a financial and constitutional crisis, emerging stronger internally and fiscally, though reduced in membership. The Constitution of 1974 made all the major offices and the three divisional committees elective, and wise fiscal management has kept us in the black.

Having survived the critical 1970s and muddled through the 1980s, the historical profession and the AHA face in the 1990s another turn of the historical kaleidoscope. The problem now is not too few jobs for too many historians but the reverse. According to a recent article in *Perspectives* by Richard Kohn, the number of history undergraduate majors graduating from American colleges declined 62 percent between 1970 and 1986, and in the same period Ph.D.s in history declined by almost 50 percent. Thus both our numbers and our academic audience have been sharply reduced in the past two decades.¹ Historians and history departments have now adjusted to a future of lowered status and lowered expectations within the academic community. But a study just completed by William Bowen and Julie Ann Sosa for the Mellon Foundation concludes that, in the humanities, including history, by the year 2000, the academic market will demand twice the number of Ph.D.s we are now preparing.² The dedicated scholars will get their deserved reward, but the profession seems destined to face a new crisis of lowered standards as we will be forced to put less qualified faculty into the classrooms.

History as a discipline has also recently experienced self-doubt and division. As the veteran historian Theodore S. Hamerow warns us in a recent, searching study, to society at large "the methodology of historical scholarship appears inadequate for an understanding of the world in which we live."³ Within the academic arena, historians are challenged by social-science disciplines less plagued by self-doubt. In Professor Hamerow's rather pessimistic analysis, however, the chief source of history's present vulnerability is internal. He contends that history, once dominated by gifted amateurs, has lost much of its audience and much of its touch with common human experience by going academic, retreating to the ivory tower, divorcing itself from the everyday realities that history is supposed to explain. Simultaneously, research has become narrower, more technical and specialized. The unity of history, the synoptic view of human experience, has been sacrificed to the compartmentalization of the discipline into geographical and topical sub-specialties. The social historian has little to say to the economic, political, or diplomatic historian, and vice versa. History never spoke in a single tongue, but now it speaks in a babble of tongues.

Perhaps Hamerow's view of the state of the historical discipline is too negative. Research not only in history but in all scholarly disciplines has always proceeded by dividing large subjects into manageable segments. Nevertheless, whether we perceive of history as being segmented into increasingly narrow sub-specialties or as divided into two warring camps, the New History and the Old,⁴ internal divisions

¹ Richard H. Kohn, "The Future of the Historical Profession," *American Historical Association Perspectives*, 27 (November 1989): 8.

² News item, *New York Times*, September 13, 1989; editorial, *Washington Post*, September 19, 1989, A26.

³ Theodore S. Hamerow, *Reflections on History and Historians* (Madison, Wis., 1987), 39–75.

⁴ See Gertrude Himmelfarb, *The New History and the Old* (Cambridge, Mass., 1987); Theodore S. Hamerow, Gertrude Himmelfarb, Lawrence W. Levine, Joan Wallach Scott, and John E. Toews, "AHR Forum: The Old History and the New," *AHR*, 94 (June 1989): 654–98.

are clearly taking a toll of the common purposes and interests of historians and are weakening its hold on the general reading public.

While academic history is plagued by self-doubt and division, we are told that the nation's youth is growing up ignorant of history. The teaching of social studies is falling on deaf ears. The educational historian Diane Ravitch and a collaborator, Chester E. Finn, Jr., conducted a national survey of 8,000 seventeen-year-olds, testing their knowledge of history and literature. The sample was representative of both sexes, different races, and all regions, and was drawn from both public and private schools. The seventeen-year-olds could answer correctly only 54 percent of the history questions and 52 percent of the literature questions. Only 32 percent knew that the Civil War occurred between 1850 and 1900, and 70 percent could not identify the Magna Carta. What these students knew, according to the survey, was mostly derived from reinforcement of school instruction by movies, television, and other media of popular culture. They remembered very little of what they were taught only in school.⁵

Others have also sounded the alarm. E. D. Hirsch, Jr., in his recent best seller, *Cultural Literacy*, concludes that the framework of knowledge required to make sense of particular bits of information is not being conveyed to our high school graduates.⁶ He recommends that schools abandon the learning-skills approach and begin teaching substantive knowledge in history, literature, mathematics, and science. Lynne Cheney, director of the National Endowment for the Humanities, speaks of a "generation at risk" of losing its memory of the past.⁷

There have been a number of earlier cries of alarm that have not resulted in any serious reforms, but that fact should not be allowed to minimize present public ignorance of our cultural heritage. This ignorance and indifference has alarming implications for the future of our nation and our historical profession. We must not have been doing our work very well. While we engage in academic trivial pursuits, there is a crying need for, and even occasionally a cry for, our presence in the struggle for reform of social studies teaching in the schools.

Amid all this gloom and doom, there is a ray of sunshine. The adult American public has, over the past decade or so, shown increased interest in the history being offered not in the classroom but through various public media. The public cannot get enough of the historical documentaries shown on television. As Richard Kohn says, "Interest in historical museums and in collecting historical objects, in genealogical research, reconstructions, the preservation of historic buildings and sites, has skyrocketed."⁸ Public historians have generally done more than academic historians to serve and promote this popular interest in history.

The question we must now consider is how well the American Historical Association is prepared to lead the historical profession through the pitfalls and into the opportunities that lie ahead in the 1990s. The AHA is the oldest and largest institution in the profession, but there are historians who are ready to abandon the AHA as an anachronism. They say we are trying to cast too wide a net over an expanded and fragmented profession, that the sessions at our annual meeting do not adequately serve the need for dialogue within their topical and

⁵ Diane Ravitch and Chester E. Finn, Jr., *What Do Our 17-Year-Olds Know? A Report on the First National Assessment of History and Literature* (New York, 1987).

⁶ E. D. Hirsch, Jr., *Cultural Literacy: What Every American Needs to Know* (Boston, Mass., 1987).

⁷ Lynne V. Cheney, *American Memory: A Report on the Humanities in the Nation's Public Schools* (Washington, n.d. [1988]), 6-7, 15-20.

⁸ Kohn, "Future of the Historical Profession," 8.

regional specialties, and that *AHR* reviews are too brief and selective to treat adequately the specialized literature that matters most to them. In other words, perhaps the profession has outgrown the AHA.

I do not believe that the AHA has been giving adequate answers to these doubters and skeptics, but I believe there are adequate answers. For example, anyone who reads the *AHR* regularly cannot help admiring the imaginative efforts of the editors to highlight central controversies within the profession. Our program committees for the past several years have been emphasizing comparative sessions that promote dialogue between specialties. And our various working committees perform an essential role in maintaining professional standards and protecting the interests of historians in such matters as access to documents, investigations of charges of plagiarism, and promotion of international and interdisciplinary scholarly interchange.

Nevertheless, those historians who have refused to join the AHA or allowed their membership to lapse have, in effect, voted no on the question of its survival and prosperity. When I sought as president-elect, in conjunction with the AHA committee on committees, to appoint the James H. Breasted Prize committee in ancient history, I was shocked to find how few of the eminent scholars our committee approached were AHA members. Similarly, too few East Asian historians or Africanists bother to join the AHA, and there are many other such gaps in our membership.

Not only narrow specialists but several other large groups are "staying away in droves" from the AHA, those who feel that the AHA speaks neither for them nor to them. These include public historians, both those in government agencies and in state and local historical societies, professors in four-year colleges and community colleges, and high school teachers of history and social studies. They tend to see the AHA as an elite organization of the professoriate, in which too large a share of the attention, recognition, and reward goes to professors in Ivy League schools and the most prestigious graduate schools. In many respects, this is not a fair indictment. After all, our principal offices are elective, and the committees that write the slates of committee assignments are themselves elective. Furthermore, my recent close acquaintance with the executive director and his staff and with the many committees of the AHA convinces me that their concerns and their efforts are those of the entire historical profession, not just an elite part of it.

Still, even if I do not see evidence of a deliberate or conspiratorial elitism in its recent history, the AHA tends to reflect the views of that part of the profession identified most strongly with the professoriate. Professors achieve status, tenure, promotion, and movement to more elite universities through the writing of monographs, most of them so narrow in scope, so technical in treatment, that only other professors appreciate them. Historians have made the monograph the *sine qua non* of professional recognition and reward. This leaves out in the cold all of those historians whose primary work is teaching undergraduates, informing the general public, or helping social studies teachers do a better job of enlightening the young.

Even though the picture I have painted of the historical profession and the AHA is mostly from the darker side of the palette, it is not my purpose to urge abandonment of the AHA. It has been an unsatisfactory association for some historians, but it is the only organization capable of leading and unifying a fragmented profession. What I propose instead, and it is not an original thought, is to urge a movement of the AHA from the learned society model I have been

describing to a professional association model. We do not need to abandon the learned society aspects of the AHA, its maintenance of standards and promotion of scholarship, but we need to go further in the direction of serving the cause of history and interests of historians in a more comprehensive way.

Three years ago, the AHA began a self-study by appointing the Ad Hoc Committee on the Future of the American Historical Association. Meeting under the joint chairmanship of Richard Kohn and myself, this committee recommended twenty changes intended to strengthen the AHA and make it a more effective instrument of the will and interests of the historical profession.⁹ These recommendations are the basis of five goals to which I shall urge you, the members of the AHA, to give priority in the next few years.

In the first place, the AHA needs to carry on a membership campaign. Alone of all the professional associations to which I belong, the AHA until this week has had no membership committee, no group of volunteers among its membership engaged in actively recruiting among their colleagues for new members or the renewal of lapsed memberships. Two staff members, not themselves historians, have done all the work of solicitation and recruitment. They do their work efficiently, and our numbers have grown slightly in recent years to about 13,000 individual members. But think of the missed potential membership. There are at least twice that number of historians with doctorates in the United States, and five times as many with masters' degrees, not to mention high school teachers and history buffs whom we might attract. Unless the AHA takes positive action, it may suffer a drastic loss of membership such as the American Medical Association has suffered in recent decades. Simply appointing a membership committee will not automatically bring results, of course. Nevertheless, it is worth trying and could make a difference. With a budget for mailing costs, with an imaginative program for soliciting among well-chosen target groups, and with the help of our eighty affiliated historical societies, we might be able to increase our ranks considerably and thus enhance our influence on the profession, on government, the school system, and society in general. Two days ago, the AHA Council took action to establish the machinery for a membership campaign, but it will require strong support from AHA members to be effective.

Secondly, the AHA needs to make itself more attractive to talented amateurs and public historians. These are not only the most rapidly growing sectors of our profession but the ones doing the most to interest the general reading and viewing public in our field of study. It is museum exhibitors, park rangers, historical filmmakers, and popular historians like Barbara Tuchman and David McCullough who keep history alive in the public mind. And how has the historical profession responded to their contributions? With a few exceptions, we have kept them outside the magic circle of office and honor. They serve on few committees, seldom are nominated for elective office in our professional organizations, and compete every year for a single book prize. We academic historians who control the profession have become, like the Chinese mandarins, increasingly out of touch with the real world.

This divorcement of academic historians from popular history has not always prevailed. In the first half-century of the AHA, such distinguished nonacademic historians as James Ford Rhodes, Alfred T. Mahan, and Theodore Roosevelt

⁹ The committee's report was published under the title, "We Have Seen the Future and It Needs Work," in *American Historical Association Perspectives*, 26 (September 1988): 1, 6, 8-9.

served as president. In the second half-century of our organization, however, no nonacademic historian has been president of the AHA. I can even think of a few academic historians who had the bad taste to write best sellers and have ever since been banished from the profession's roll of honor. We need to remedy this second-class citizenship for public historians and other nonacademic historians. We already give them a token amount of recognition and reward, but we need to extend this and break down the barriers between academic and nonacademic history. We do not need to lower our vaunted standards as a learned society. We simply need to recognize that the research monograph is not the only way to "do" history, nor is the scholarly audience the only appropriate audience.

The third change I urge is a deeper involvement by members of the American Historical Association in the present crisis of social studies in the schools. The association is already officially involved through its teaching division committee and through the service of several of its members on the Bradley Commission¹⁰ and on the National Commission on Social Studies in the Schools.¹¹ Moreover, for the past year, the AHA and the Organization of American Historians have sponsored a joint conference on social studies in the schools, meeting twice a year with representatives of seven or eight groups active in the effort to improve the social studies curriculum and certification standards of social studies teachers. What we need now is to involve more of our rank-and-file members in this chancy but important work. It is not that we need to teach teachers how to teach. The social studies teacher does more teaching by ten o'clock in the morning than the average professor does all day—that is, on his or her teaching day. What social studies teachers need from us professional historians is access to our knowledge and expertise in subject matter, on an ongoing basis.

Many more historians need to be willing to spend a little time with elementary and high school teachers, talking about the latest and most interesting scholarship in their fields. The problem is neither lack of such expertise nor lack of an appreciative audience of teachers. As David Van Tassel, the founder and president of National History Day, explained at a meeting of the AHA-OAH joint committee on social studies, the problem is that, under the present rules of academic history, professors are rewarded by tenure and promotion for scholarly research and writing but not for efforts to help history teachers in the schools or any other form of public service. For their labor in preparing and delivering speeches to schoolteachers, they get nothing, except perhaps a raised eyebrow of doubt about their professional priorities. This leaves the work of trying to help schoolteachers to those who are willing to do it without reward, as a labor of love. Unfortunately, there are not enough great lovers to go around. Now, this is obviously a problem without an easy solution. We cannot expect the solution to come from the academic community, which already has a different set of standards and rewards. The leadership must come, if it does come, from professional associations such as the AHA. As the organizational guardian of the historical profession, we ought to be able to see that we have a stake in how well history is taught in our schools. Then we need to persuade our membership and the history departments in our colleges

¹⁰ See the Bradley Commission on History in Schools, *Building a History Curriculum: Guidelines for Teaching History in Schools* (Washington, 1988), and its fuller statement in *Historical Literacy: The Case for History in American Education* (New York, 1989).

¹¹ See *Charting a Course: Social Studies for the 21st Century: A Report of the Curriculum Task Force of the National Commission on Social Studies in the Schools* (Washington, D.C., 1989). David Jenness is preparing for the commission a history of the reform movement in social studies teaching.

and universities that our presence is needed in the public debate now going on about the curricular content and character of our schools. The officers of the AHA cannot bring this about, but the members can.

This brings me to the fourth step I believe the AHA should take to place itself in a leadership role in our profession. We need to broaden our commitment to the advocacy of history as a discipline and as a profession. As most of you know, the AHA is the largest of forty-eight historical and archival groups that support the National Coordinating Committee for the Promotion of History. Its office is at the AHA headquarters in Washington, and, although it has state coordinating committees in twenty-eight states, it largely consists of a single lobbyist, plus such volunteers as she is able to muster, and confines its limited resources to lobbying the federal government. The National Coordinating Committee does an excellent job in its limited sphere, and history now has a presence when government decisions are made about archival access, historical museums, government agency history programs, and the public funding of historical research projects. There are other arenas, however, where key decisions are being made without the organized presence of the historical community, out beyond the Beltway in the state and local governments. If we are to have a real impact as professional historians on the way the schools present history, we need a network scattered across many states, ready to present the case for accurate, well-informed, exciting history to school boards, state legislatures, departments of education, and textbook commissions. Similarly, if we have a network, we can join with archivists in shaping the decisions of state and local governments about record-keeping and record-preserving, key decisions about the future availability of historical evidence. This network in the states can consist of volunteers, but we need at least one full-time staff member coordinating their efforts and sharing information.

The changes I have recommended so far will require additional funding from the AHA budget, although the cost of an expanded advocacy program might expect to be shared by the forty-seven other groups that sponsor the National Coordinating Committee. The modest increase in dues proposed for next year will only cover present expenses with a correction for inflation. Nearly all of the AHA's endowed funds are specifically bound by their deeds of gift to particular objects, such as book prizes and research grants. For many years, the AHA has relied on foundation grants—soft money—to fund innovative programs. We should continue to apply for these grants and hope that the membership campaign will bring in additional revenues. These sources, however, are too uncertain to rely on. This brings me to the fifth change I believe the AHA should make, one suggested not by the report of the Ad Hoc Committee on the Future of the AHA but by an AHA member in the Washington, D.C., area, James Banner, Jr. After reading our committee report, Dr. Banner wrote, pointing out an omission. He suggested that the AHA undertake an endowment campaign for the general purposes of the association. I heartily endorse this suggestion. The AHA should solicit its members, and outsiders interested in history, for contributions toward a general endowment that would free the association from complete dependence on foundation grants for initiating and sustaining programs vital to the welfare of the historical profession. This endowment would make it possible to save good initiatives from dying on the vine when foundations refuse to support them or abandon them after two or three years. The chief trouble with this idea is that our sister association, the Organization of American Historians, had the idea first. We have now allowed the OAH a grace period of two years to launch its own Fund for American History.

The AHA can now begin, in early 1990, the planning stage for a general endowment campaign to be launched in 1991, and I so recommend.

These five changes, immediately begun, will not radically change the character of the American Historical Association. It will still remain the great learned society of our profession. These changes will, however, move us in the direction of better serving the needs of our broadening and once again growing profession, and they will also begin to serve better than in the past the large literate public that is thirsting for the history we can offer it if we can only punch our way out of our academic paper bag. I repeat my recommendations for emphasis: a membership campaign, a bridging of the gap between academic historians on the one hand and public historians or gifted amateur historians on the other, a deeper involvement with the needs and concerns of teachers of social studies in the schools, a larger advocacy role in the promotion of history's interests, and an AHA capital fund for general purposes. These may not be the only changes we ought to make in the near future, but I believe they would be a good start in the right direction. They can only come about if you, the members of the American Historical Association, give them your active support.

Gracia Real de Santa Teresa de Mose: A Free Black Town in Spanish Colonial Florida

JANE LANDERS

FOR TOO LONG, historians have paid little attention to Spain's lengthy tenure in the South.¹ As a result, important spatial and temporal components of the American past have been overlooked. Recent historical and archaeological research on the free black town of Gracia Real de Santa Teresa de Mose, located in northeast Spanish Florida, suggests ways in which Spanish colonial records might illuminate these neglected aspects of the Southern past.² Because of this black town's unusual origins and political and military significance, Spanish bureaucrats documented its history with much care.

Gracia Real de Santa Teresa de Mose, hereafter referred to as Mose, was born of

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¹ An early classic that examined the triracial Southern frontier was Verner W. Crane, *The Southern Frontier, 1670–1732* (New York, 1981), but, as Peter Wood noted in his historiographic review, "I Did the Best I Could for My Day": The Study of Early Black History during the Second Reconstruction, 1960–1976," *William and Mary Quarterly*, 3d ser., 35 (1978): 185–225, few scholars followed Crane's lead. The difficulty of the sources deterred some from crossing the cultural and linguistic frontier into Florida, but Latin Americanists have also neglected what were the northern boundaries of the Spanish empire. The "Borderlands" school pioneered by Herbert Bolton produced a number of important studies, but these focused primarily on the southwestern areas of the present-day United States. See Herbert E. Bolton, *The Spanish Borderlands, A Chronicle of Old Florida and the Southwest* (Toronto, 1921); and Herbert E. Bolton and Mary Ross, *The Debatable Land* (Berkeley, Calif., 1925). For a review of these borderland studies, see David Weber, "John Francis Bannon and the Historiography of the Spanish Borderlands," *Journal of the Southwest*, 29 (Winter 1987): 331–63.

² Scholars who have attempted to explore the African experience in northern America through Spanish sources include John TePaske, "The Fugitive Slave: Intercolonial Rivalry and Spanish Slave Policy, 1687–1764," in Samuel Proctor, ed., *Eighteenth-Century Florida and Its Borderlands* (Gainesville, Fla., 1975), 1–12; Jack D. L. Holmes, "The Role of Blacks in Spanish Alabama: The Mobile District, 1780–1813," *Alabama Historical Quarterly*, 37 (Spring 1975): 5–18; Gilbert Din, "Cimarrones and the San Malo Band in Spanish Louisiana," *Louisiana History*, 21 (Summer 1980): 237–62; Jack D. Forbes, "Black Pioneers: The Spanish-Speaking Afroamericans of the Southwest," *Phylon*, 27 (1966): 233–46; Peter Stern, "Social Marginality and Acculturation on the Northern Frontier of New Spain" (Ph.D. dissertation, University of California, Berkeley, 1984); and Kimberly Hanger, "Free Blacks in Spanish New Orleans—The Transitional Decade, 1769–1779" (Masters thesis, University of Utah, 1985). Gwendolyn Midlo Hall has a detailed study of Africans in colonial Louisiana forthcoming that is drawn from Spanish as well as French sources.

the initiative and determination of blacks who, at great risk, manipulated the Anglo-Spanish contest for control of the Southeast to their advantage and thereby won their freedom. The settlement was composed of former slaves, many of West African origin, who had escaped from British plantations and received religious sanctuary in Spanish Florida. Although relatively few in number (the community maintained a fairly stable size of about 100 people during the quarter-century between 1738 and 1763, while St. Augustine's population grew from approximately 1,500 people in the 1730s to approximately 3,000 by 1763), these freedmen and women were of great contemporary significance.³ By their "theft of self," they were a financial loss to their former owners, often a serious one.⁴ Moreover, their flight was a political action, sometimes effected through violence, that offered an example to other bondsmen and challenged the precarious political and social order of the British colonies. The runaways were also important to the Spanish colony for the valuable knowledge and skills they brought with them and for the labor and military services they performed.⁵ These free blacks are also historiographically significant; an exploration of their lives sheds light on questions long debated by scholars, such as the relative severity of slave systems, the varieties of slave experiences, slave resistance, the formation of a Creole culture, the nature of black family structures, the impact of Christianity and religious syncretism on African-American societies, and African-American influences in the "New World."⁶

Although a number of historians have alluded to the lure of Spanish Florida for runaway slaves from the British colonies of South Carolina and Georgia, few have examined what became of the fugitives in their new lives or the implications of their

³ Theodore G. Corbett, "Migration to a Spanish Imperial Frontier in the Seventeenth and Eighteenth Centuries: St. Augustine," *Hispanic American Historical Review*, 54 (August 1974): 419–20. Corbett noted that St. Augustine, the largest of the borderland settlements, also had the most blacks, slave and free, in the Spanish borderlands. As late as 1763, St. Augustine was larger than any other town in the southern colonies except Charleston. See Theodore G. Corbett, "Population Structure in Hispanic St. Augustine, 1629–1763," *Florida Historical Quarterly*, 54 (July 1975–April 1976): 268.

⁴ Peter Wood, *Black Majority; Negroes in Colonial South Carolina from 1670 through the Stono Rebellion* (New York, 1974), 239–68; Philip D. Morgan, "Colonial South Carolina Runaways: Their Significance for Slave Culture," in *Slavery and Abolition*, 6 (December 1985): 57–78; Darrett Rutman and Anita Rutman, *A Place in Time: Middlesex County, Virginia, 1650–1750* (New York, 1984), 180–87.

⁵ The role of Africans as cultural agents is discussed in Wood, *Black Majority*, 35–63, 95–130. Also see Daniel Littlefield, *Rice and Slaves, Ethnicity and the Slave Trade in Colonial South Carolina* (Baton Rouge, La., 1981), 98–99. Wood also pointed out that "in literally every conflict in eighteenth-century South Carolina there were Negroes engaged on both sides"; Wood, *Black Majority*, 128–29.

⁶ Frank Tannenbaum, *Slave and Citizen* (New York, 1946). Tannenbaum's early view that institutional protections benefited slaves in Hispanic areas was challenged by scholars who found economic determinants of slave treatment more significant. See Eugene Genovese, "The Treatment of Slaves in Different Countries: Problems in the Application of the Comparative Method," in Laura Foner and Eugene Genovese, eds., *Slavery in the New World* (Englewood Cliffs, N.J., 1969), 202–10; Marvin Harris, *Patterns of Race in the Americas* (New York, 1964). Historians who have reviewed Spanish racial prejudice and discriminatory regulations include Lyle McAlister, "Social Structure and Social Change in New Spain," *Hispanic American Historical Review*, 43 (April 1963): 349–70; Magnus Mörner, *Race Mixture in the History of the Americas* (Boston, 1967); and Leslie B. Rout, Jr., *The African Experience in Spanish America, 1502 to the Present* (Cambridge, 1976). On the varieties of slave experiences, see Sidney M. Mintz and Richard Price, *An Anthropological Approach to the Afro-American Past: A Caribbean Perspective* (Philadelphia, 1976). On resistance in Latin America, see Richard Price, ed., *Maroon Societies, Rebel Slave Communities in the Americas* (Garden City, N.Y., 1973). On the formation of Creole cultures, see Charles Joyner, *Down by the Riverside, A South Carolina Slave Community* (Urbana, Ill., 1984). On black families, see Ira Berlin, "Time, Space and the Evolution of Afro-American Society on British Mainland North America," *AHR*, 85 (June 1980): 44–78. On black religion and African cultural retentions in the "New World," see Robert Farris Thompson, *Flash of the Spirit: African and Afro-American Art and Philosophy* (New York, 1984); and Margaret Washington Creel, "A Peculiar People": *Slave Religion and Community-Culture among the Gullahs* (New York, 1988). For an interesting comparison of African and British world views and attitudes, see Mechal Sobel, *The World They Made Together: Black and White Values in Eighteenth-Century Virginia* (Princeton, N.J., 1987).

presence in the Spanish province.⁷ The Spanish policy regarding fugitive slaves in Florida developed in an ad hoc fashion and changed over time to suit the shifting military, economic, and diplomatic interests of the colony as well as the metropolis. Although the Spanish crown preferred to emphasize religious and humane considerations for freeing slaves of the British, the political and military motives were equally, if not more, important. In harboring the runaways and eventually settling them in their own town, Spanish governors were following Caribbean precedents and helping the crown to populate and hold territory threatened by foreign encroachment.⁸ The ex-slaves were also served by this policy. It offered them a refuge within which they could maintain family ties. In the highly politicized context of Spanish Florida, they struggled to maximize their leverage with the Spanish community and improve the conditions of their freedom. They made creative use of Spanish institutions to support their corporate identity and concomitant privileges.⁹ They adapted to Spanish values where it served them to do so and thereby gained autonomy. They also reinforced ties within their original community through intermarriage and use of the Spanish mechanism of godparenthood (*compadrazgo*). Finally, they formed intricate new kin and friendship networks with slaves, free blacks, Indians, "new" Africans, and whites in nearby St. Augustine that served to stabilize their population and strengthen their connections to that Hispanic community.¹⁰

THAT RUNAWAYS BECAME FREE in Spanish Florida was not in itself unusual. Frank Tannenbaum's early comparative work shows that freedom had been a possibility for slaves in the Spanish world since the thirteenth century. Spanish law granted slaves a moral and juridical personality, as well as certain rights and protections not found in other slave systems. Among the most important were the right to own property, which in the Caribbean evolved into the right of self-purchase, the right to personal security, prohibitions against separating family members, and access to the courts. Moreover, slaves were incorporated into the Spanish church and received its sacraments, including marriage. Slaves in the Hispanic colonies were subject to codes based on this earlier body of law.¹¹ Eugene Genovese and others have persuasively argued that the ideals expressed in these slave codes should not be accepted as social realities, and it seems obvious that colonials observed these laws in their own fashion—some in the spirit in which they were written and others

⁷ Irene Wright, "Dispatches of Spanish Officials Bearing on the Free Negro Settlement of Gracia Real de Santa Teresa de Mose," *Journal of Negro History*, 9 (1924): 144–93; TePaske, "Fugitive Slaves"; Luis Arana, "The Mose Site," *El Escribano*, 10 (April 1973): 50–62; Kenneth Wiggins Porter, *The Negro on the American Frontier* (New York, 1971); Jane Landers, "Spanish Sanctuary: Fugitive Slaves in Florida, 1687–1790," *Florida Historical Quarterly*, 62 (September 1984): 296–313; Larry W. Kruger and Robert Hall, "Fort Mose: A Black Fort in Spanish Florida," *The Griot*, 6 (Spring 1987): 39–48.

⁸ Lyle N. McAlister, *Spain and Portugal in the New World, 1492–1700* (Minneapolis, Minn., 1984), 133–52.

⁹ On corporate privileges of the Spanish militias, see Lyle N. McAlister, *The "Fuero Militar" in New Spain, 1764–1800* (Gainesville, Fla., 1957); Herbert S. Klein, "The Colored Militia of Cuba: 1568–1868," *Caribbean Studies*, 6 (July 1966): 17–27; Allan J. Kuethe, "The Status of the Free Pardo in the Disciplined Militia of New Granada," *Journal of Negro History*, 56 (April 1971): 105–15; Roland C. McConnell, *Negro Troops of Antebellum Louisiana—A History of the Battalion of Free Men of Color* (Baton Rouge, La., 1968).

¹⁰ On the function and meaning of godparents, see George M. Foster, "Cofradía and Compadrazgo in Spain and Spanish America," *Southwestern Journal of Anthropology*, 9 (1953): 1–28; Sidney W. Mintz and Eric Wolf, "An Analysis of Ritual Co-Parenthood (Compadrazgo)," *Southwestern Journal of Anthropology*, 6 (1950): 341–67.

¹¹ Tannenbaum, *Slave and Citizen*.

not at all.¹² Nevertheless, the acknowledgement of a slave's humanity and rights, and the lenient attitude toward manumission embodied in Spanish law and social practices, made it possible for a significant free black class to exist in the Spanish world.¹³

Although the Spanish legal system permitted freedom, the crown assumed that its beneficiaries would live among the Spaniards, under the supervision of white townspeople (*vecinos*). While the crown detailed its instructions regarding the physical layout, location, and function of white and Indian towns, it made no formal provisions for free black towns. But Spanish colonizers throughout the Americas were guided by an urban model. They depicted theirs as a civilizing mission and sought to create public order and righteous living by creating towns. Urban living was believed to facilitate religious conversion, but, beyond that, Spaniards attached a special value to living a *vida política*, believing that people of reason distinguished themselves from nomadic "barbarians" by living in stable urban situations.¹⁴ Royal legislation reflected a continuing interest in reforming and settling so-called vagabonds of all races within the empire. The primary focus of reduction efforts was the Indians, but, as the black and mixed populations grew, so too did Spanish concerns about how these elements would be assimilated into "civilized" society. The "two republics" of Spaniards and Indians gave way to a society of castes, which increasingly viewed the unforeseen and unregulated groups with hostility. Spanish bureaucrats attempted to count these people and to limit their physical mobility through increasingly restrictive racial legislation. Officials prohibited blacks from living unsupervised or, worse, among the Indians. Curfews and pass systems developed, as did proposals to force unemployed blacks into fixed labor situations.¹⁵ The crown also recognized with alarm the increased incidence of *cimarronage*, slaves fleeing Spanish control. Communities of runaway blacks, mulattos, Indians, and their offspring were common to all slaveholding societies, but they challenged the Spanish concept of civilized living, as well as the hierarchical racial and social order the Spaniards were trying to impose. Despite repeated military efforts, the Spaniards were no more successful than other European powers at eradicating such settlements.¹⁶

Paradoxically, it was in this context of increasing racial animosity that Spanish officials legitimized free black towns. These towns appeared in the seventeenth and eighteenth centuries in a region described by one scholar as the "Negroid

¹² Genovese, "Treatment of Slaves"; Mörner, *Race Mixture*; Rout, *African Experience*. For a study of the law in practice, see Norman A. Meiklejohn, "The Observance of Negro Slave Legislation in Colonial Nueva Granada" (Masters thesis, Columbia University, 1968).

¹³ Hanger, "Free Blacks"; David W. Cohen and Jack P. Greene, eds., *Neither Slave nor Free, The Freedmen of African Descent in the Slave Societies of the New World* (Baltimore, Md., 1972); Landers, "Spanish Sanctuary"; Ira Berlin, *Slaves without Masters—The Free Negro in the Antebellum South* (New York, 1974), 108–32; Lyman L. Johnson, "Manumission in Colonial Buenos Aires," *Hispanic American Historical Review*, 59 (1979): 258–79; Frederick Bowser, "Free Persons of Color in Lima and Mexico City: Manumission and Opportunity, 1580–1650," in Stanley Engerman and Eugene D. Genovese, eds., *Slavery in the Western Hemisphere: Quantitative Studies* (Princeton, N.J., 1974), 331–68.

¹⁴ Richard Morse elegantly analyzed the concept of the *ciudad perfecta* and Spanish efforts to reproduce it in the New World in his chapter, "A Framework for Latin American Urban History" in Jorge Hardoy, ed., *Urbanization in Latin America: Approaches and Issues* (Garden City, N.Y., 1975), 57–107.

¹⁵ Richard Konezke, "Estado y sociedad en las Indias," *Estudios Americanos*, 3 (1951): 33–58; Rolando Mellafe, *Negro Slavery in Latin America* (Berkeley, Calif., 1975), 109–17.

¹⁶ Meiklejohn, "Observance of Negro Slave Legislation," 103–14, 295–306; Carlos Federico Guillot, *Negros rebeldes y negro cimarrones: Perfil afroamericano en la historia del Nuevo Mundo durante el siglo XVI* (Buenos Aires, 1961); Miguel Acosta Saignes, *Vida de los esclavos negros en Venezuela* (Caracas, 1967), 249–84; R. K. Kent, "Palmares: An African State in Brazil," *Journal of African History*, 6 (1965): 161–75; Carlos Larrazábal Blanco, *Los negros y la esclavitud en Santo Domingo* (Santo Domingo, 1967).

littoral"—the sparsely populated and inhospitable coastal areas of the Caribbean.¹⁷ Faced with insurmountable problems and lacking the resources to "correct" them, the Spanish bureaucracy proved flexible and adaptable. When maroon communities such as those described by Colin Palmer and William Taylor in Mexico were too remote or intractable to destroy, the Spaniards granted them official sanction.¹⁸ The Spanish governor of Venezuela once chartered a free black town to reward pacification of lands held by hostile Indians.¹⁹ Mose was established as a buffer against foreign encroachment and provides a third model of free black town formation.²⁰

The experience of the residents of Mose was in many ways shaped by Caribbean patterns. Declining Indian populations, a Spanish disdain for manual labor, and the defense requirements of an extended empire had created an early demand for additional workers. Blacks cleared land and planted crops, built fortifications and domestic structures, and provided a wide variety of skilled labor for Spanish colonists. By the sixteenth century, they had become the main labor force in Mexican mines and on Caribbean plantations. Also by that time, the Spanish had organized them into militia companies in Hispaniola, Cuba, Mexico, Cartagena, and Puerto Rico.²¹ In Florida, too, Spaniards depended on Africans to be their laborers and to supplement their defenses. Black laborers and artisans helped establish St. Augustine, the first successful Spanish settlement in Florida, and a black and mulatto militia was formed there as early as 1683.²²

Florida held great strategic significance for the Spanish: initially, for its location guarding the route of the treasure fleets, later, to safeguard the mines of Mexico from the French and British. The colony was a critical component in Spain's Caribbean defense, and, when British colonists established Charles Town in 1670, it represented a serious challenge to Spanish sovereignty.²³ No major response by the weakened Spanish empire was feasible, but, when the British incited their Indian allies to attack Spanish Indian missions along the Atlantic coast, the Spaniards initiated a campaign of harassment against the new British colony. In 1686, a Spanish raiding party including a force of fifty-three Indians and blacks attacked Port Royal and Edisto. From the plantation of Governor Joseph Morton, they carried away "money and plate and thirteen slaves to the value of [£]1500." In subsequent negotiations, the new governor of Carolina, James Colleton, demanded the return of the stolen slaves as well as those "who run dayly into your towns," but

¹⁷ Leon Campbell used this term in his article, "The Changing Racial and Administrative Structure of the Peruvian Military under the Later Bourbons," *The Americas*, 32 (July 1975): 117–33.

¹⁸ Colin Palmer, *Slaves of the White God-Blacks in Mexico, 1570–1650* (Cambridge, 1976); William Taylor, "The Foundation of Nuestra Señora de Guadalupe de los Morenos de Amapa," *The Americas*, 26 (April 1970): 442–46.

¹⁹ Richard Konetzk, *Colección de documentos para la historia de la formación social de Hispano-América, 1493–1810*, 3 vols. (Madrid, 1953–58), 2: 118–20.

²⁰ For a later example of a buffer town, see John Hoyt Williams, "Trevegó on the Paraguayan Frontier: A Chapter in the Black History of the Americas," *Journal of Negro History*, 56 (October 1971): 272–83; and Germán de Granda, "Origen, función y estructura de un pueblo de negros y mulatos libres en el Paraguay del siglo XVIII (San Agustín de la Emboscada)," *Revista de Indias*, 43 (enero–junio 1983): 229–64.

²¹ Klein, "Colored Militia"; Kuethe, "Status of the Free Pardo."

²² Roster of Black and Mulatto Militia for St. Augustine, September 20, 1683, Santo Domingo (hereafter cited as SD), 266, Archivo General de Indias: Seville (hereafter cited as AGI).

²³ Crane, *Southern Frontier*, 3–17; John Jay TePaske, *The Governorship of Spanish Florida, 1700–1763* (Durham, N.C., 1964), 3–6; Verne E. Chatelain, *The Defenses of Spanish Florida, 1565–1763* (Washington, D.C., 1941).

the Spaniards refused.²⁴ These contacts may have suggested the possibility of a refuge among the enemy and directed slaves to St. Augustine, for, the following year, the first recorded fugitive slaves from Carolina arrived there. Governor Diego de Quiroga dutifully reported to Spain that eight men, two women, and a three-year-old nursing child had escaped to his province in a boat. According to the governor, they requested baptism into the "True Faith," and on that basis he refused to return them to the British delegation that came to St. Augustine to reclaim them.²⁵ The Carolinians claimed that one of Samuel de Bordieu's runaways, Mingo, who escaped with his wife and daughter (the nursing child), had committed murder in the process. Governor Quiroga promised to make monetary restitution for the slaves he retained and to prosecute Mingo, should the charges be proven.²⁶ Quiroga housed these first runaways in the homes of Spanish townspeople and saw to it that they were instructed in Catholic doctrine, baptized and married in the church. He put the men to work as ironsmiths and laborers on the new stone fort, the Castillo de San Marcos, and employed the women in his own household. All were reportedly paid wages: the men earned a peso a day, the wage paid to male Indian laborers, and the women half as much.²⁷

Florida's governors enjoyed considerable autonomy. Their dual military and political appointments, the great distance from the metropolis, and an unwieldy bureaucracy contributed to their ability to make their own decisions. In unforeseen circumstances, they improvised. But, as fugitives continued to filter into the province, the governors and treasury officials repeatedly solicited the king's guidance. Eventually, the Council of the Indies reviewed the matter and recommended approving the sanctuary policy shaped by the governors. On November 7, 1693, Charles II issued the first official position on the runaways, "giving liberty to all . . . the men as well as the women . . . so that by their example and by my liberality others will do the same."²⁸

The provocation inherent in this order increasingly threatened the white Carolinians. At least four other groups of runaways reached St. Augustine in the following decade, and, despite an early ambiguity about their legal status, the refugees were returned to their British masters only in one known example.²⁹

²⁴ Letter from Mr. Randolph to the Board, June 28, 1699, in A.S. Salley, *Records of the British Public Records Office Relating to South Carolina, 1698-1700* (Columbia, S.C., 1946), 4: 89; Crane, *Southern Frontier*, 31-33.

²⁵ "William Dunlop's Mission to St. Augustine in 1688," *South Carolina Historical and Genealogical Magazine*, 34 (January 1933): 1-30; Diego de Quiroga to the king, February 2, 1688, cited in Wright, "Dispatches," 150. Morton's stolen male slaves included Peter, Scipio, Doctor (whose name suggests a specialized function or skill), Cushi, Arro, Emo, Caesar, and Sambo. The women included Frank, Bess, and Mammy. Sambo was the Hausa name for a second son, while in Mende or Vai it meant "disgrace." Cushi may have been "Quashee," the Twi day-name for Sunday, which also came to signify "foolish" or "stupid." For a discussion of slave naming, see Wood, *Black Majority*, 181-86, among others. The men who stole the canoe were named Conano, Jesse, Jacque, Gran Domingo (Big Sunday), Cambo, Mingo, Dicque, and Robi. Wood suggests that forms of the name Jack derived from the African day-name for Wednesday, Quaco. Names of the two women and the little girl were not given. The owners of the fugitives who escaped in the canoe were: Samuel de Bordieu, Mingo, his wife and daughter; John Bird, two men; Joab Howe, one man; John Berresford, one woman; Christopher Smith, one man; Robert Cuthbert, three men. "William Dunlop's Mission," 4, 26, 28.

²⁶ "William Dunlop's Mission," 25.

²⁷ Royal officials to the king, March 3, 1689, cited in Wright, "Dispatches," 151-52.

²⁸ Royal edict, November 7, 1693, SD 58-1-26 in the John B. Stetson Collection (hereafter cited as ST), P. K. Yonge Library of Florida History, University of Florida, Gainesville (hereafter cited as PKY). Also see "William Dunlop's Mission," 1-30.

²⁹ The various petitions of Carolina fugitives gathered together by Governor Manuel de Montiano are found in SD 844, fols. 521-46, microfilm reel 15, PKY. They mention groups arriving in 1688, 1689, 1690, 1697, 1724, and 1725. Governor Joseph de Zuñiga reported that his predecessor, Governor

Carolina's changing racial balance further intensified the planters' concerns. By 1708, blacks outnumbered whites in the colony, and slave revolts erupted in 1711 and 1714. The following year, when many slaves joined the Yamassee Indian war against the British, they almost succeeded in exterminating the badly outnumbered whites. Indians loyal to the British helped defeat the Yamassee, who with their black allies headed for St. Augustine. Although the Carolina Assembly passed harsh legislation designed to prevent further insurrections and control the slaves, these actions and subsequent negotiations with St. Augustine failed to deter the escapes or effect the reciprocal return of slaves. British planters claimed that the Spanish policy, by drawing away their slaves, would ruin their plantation economy. Arthur Middleton, Carolina's acting governor, complained to London that the Spaniards not only harbored their runaways but sent them back in the company of Indians to plunder British plantations. The Carolinians set up patrol systems and placed scout boats on water routes to St. Augustine, but slaves still made good their escapes on stolen horses and in canoes and piraguas.³⁰

In 1724, ten more runaway slaves reached St. Augustine, assisted by English-speaking Yamassee Indians. According to their statements, they were aware that the Spanish king had offered freedom to those seeking baptism and conversion.³¹ The royal edict of 1693 was still in force, and Governor Antonio de Benavides initially seems to have honored it. In 1729, however, Benavides sold these newcomers at public auction to reimburse their owners, alleging that he feared the British might act on their threats to recover their losses by force. Some of the most important citizens of St. Augustine, including the royal accountant, the royal treasurer, several military officers, and even some religious officials, thus acquired valuable new slaves.³² Others were sold to owners who took them to Havana. In justifying his actions, Benavides explained that these slaves had arrived during a time of peace with England and, further, that he interpreted the 1693 edict to apply only to the original runaways from the British colony.³³

Several of the reenslaved men were veterans of the Yamassee war in Carolina, and one of these, Francisco Menéndez, was appointed by Governor Benavides to command a slave militia in 1726. This black militia helped defend St. Augustine against the British invasion led by Colonel John Palmer in 1728, but, despite their loyal service, the Carolina refugees still remained enslaved.³⁴ Meanwhile, the Spaniards continued to send canoes of Carolina fugitives and Yamassee Indians north in search of British scalps and live slaves. Governor Middleton charged that

Laureano de Torres y Ayala, on August 8, 1697, returned six blacks and an Indian who had escaped from Charlestown that year, "to avoid conflicts and ruptures between the two governments." Joseph de Zuñiga to the king, October 10, 1699, SD 844, microfilm reel 15, fol. 542, PKY.

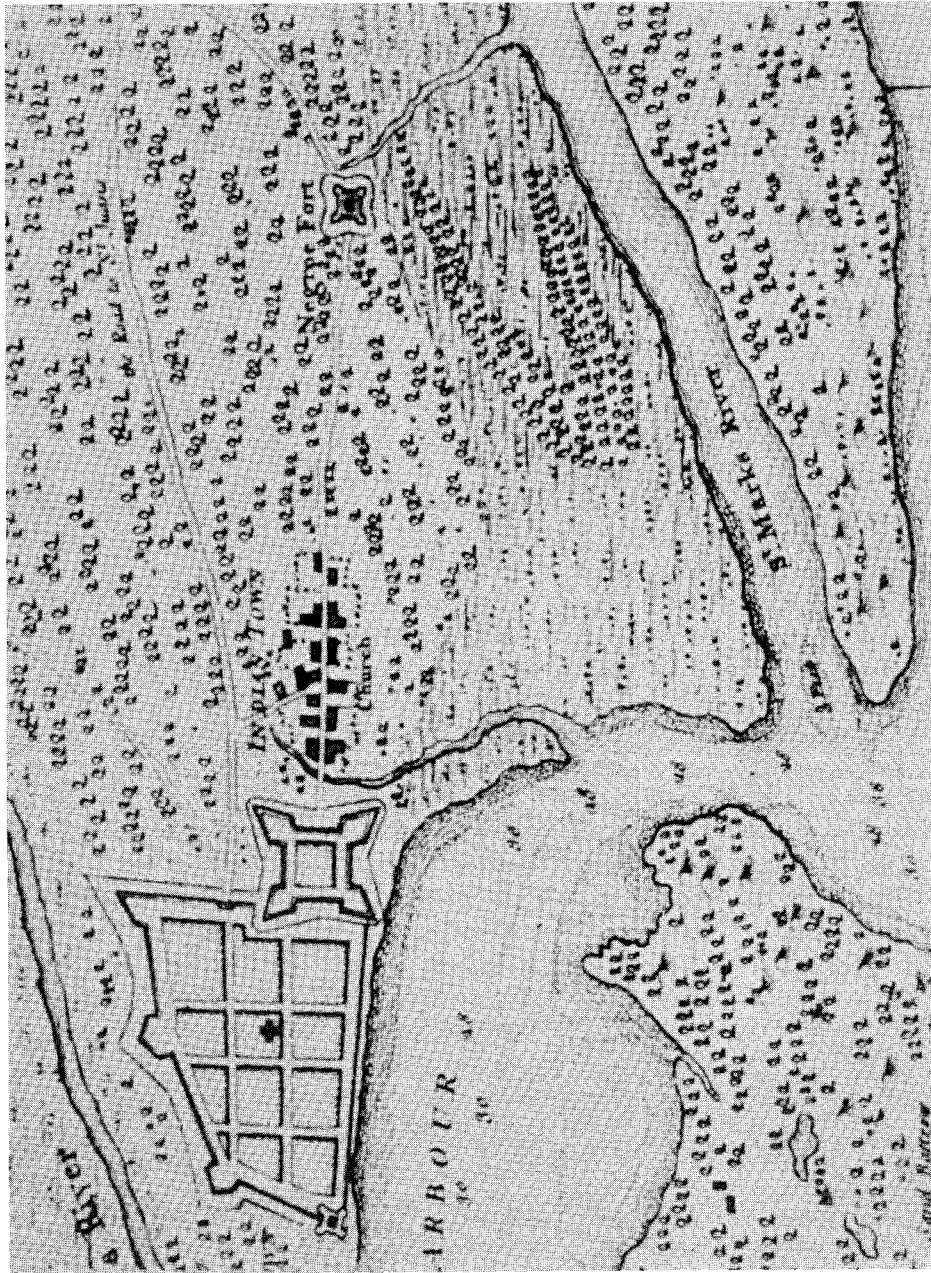
³⁰ Wood, *Black Majority*, 304–05. For a new overview of the broader demographic context, see Peter H. Wood, "The Changing Population of the Colonial South: An Overview by Race and Region, 1685–1790," in Peter H. Wood, Gregory A. Waselkov, and M. Thomas Hatley, eds., *Powhatan's Mantle: Indians in the Colonial Southeast* (Lincoln, Neb., 1989), 35–103.

³¹ Memorial of the Fugitives, 1724, SD 844, fol. 530, microfilm reel 15, PKY.

³² Governor Antonio de Benavides to the king, November 11, 1725, cited in Wright, "Dispatches," 164–66. The noted citizens who acquired the slaves filed various memorials to record their concerns about British threats to come take the slaves and the fact that British forces outnumbered Spanish. Memorial, August 26, 1729, SD 844, fols. 550–62. Governor Benavides then authorized their auction and gave the proceeds to a British envoy, Arthur Hauk. Accord, June 27, 1730, SD 844, fols. 564–66, microfilm reel 15, PKY.

³³ Consulta by the Council of the Indies, April 12, 1731, cited in Wright, "Dispatches," 166–72.

³⁴ Petition of Francisco Menéndez, November 21, 1740, SD 2658, AGI. On the role of the black militia in 1728, see TePaske, *Fugitive Slave*, 7.



MAP BY Thomas Jefferys (1699-1775), "Plan of the Town and Harbour of St. Augustin," from *A Description of the Spanish Islands and Settlements on the Coast of the West Indies* (London, 1762), plate 6. Courtesy of P. K. Yonge Library of Florida History, University of Florida. The "Negro Fort" is Mose, approximately two miles from the city of St. Augustine. This map shows the strategic location of Mose, exposed as the northernmost outpost of the Spanish empire, and the protected location of the Indian settlement. My thanks to Darci A. MacMahon at the Florida Museum of Natural History for her help with illustrations.

Governor Benavides was profiting by the slaves' sale in Havana, a charge that seems well founded.³⁵

Perhaps in response to continued reports and diplomatic complaints involving the fugitives, the crown issued two new edicts regarding their treatment. The first, on October 4, 1733, forbade any future compensation to the British, reiterated the offer of freedom, and specifically prohibited the sale of fugitives to private citizens. The second edict, on October 29, 1733, commended the blacks for their bravery against the British in 1728; however, it also stipulated that they would be required to complete four years of royal service prior to being freed. But the runaways had sought liberty, not indenture.³⁶ Led by Captain Menéndez of the slave militia, the blacks persisted in attempts to secure complete freedom. They presented petitions to the governor and to the auxiliary bishop of Cuba, who toured the province in 1735, but to no avail.³⁷ When Manuel de Montiano became governor in 1737, their fortunes changed. Captain Menéndez once more solicited his freedom, and this time his petition was supported by that of a Yamassee cacique named Jorge. Jorge related how Menéndez and three others had fought bravely for three years in the Yamassee rebellion, only to be sold back into slavery in Florida by a "heathen" named Mad Dog. Jorge condemned this betrayal of the blacks whom he stated had been patient and "more than loyal," but he did not blame Mad Dog, for he was an "infidel" who knew no better. Rather, he held culpable the Spaniards who had purchased these loyal allies.³⁸ Governor Montiano ordered an investigation and reviewed the case. On March 15, 1738, he granted unconditional freedom to the petitioners. Montiano also wrote the governor and captain general of Cuba, attempting to retrieve eight Carolinians who had been taken to Havana during the Benavides regime. At least one, Antonio Caravallo, was returned to St. Augustine, against all odds.³⁹

GOVERNOR MONTIANO ESTABLISHED THE FREEDMEN IN A NEW TOWN, about two miles north of St. Augustine, which he called Gracia Real de Santa Teresa de Mose.⁴⁰ The freedmen built the settlement, a walled fort and shelters described by the Spaniards as resembling thatched Indian huts. Little more is known about it from Spanish sources, but later British reports add that the fort was constructed of stone, "four square with a flanker at each corner, banked with earth, having a ditch without on

³⁵ Governor Arthur Middleton, June 6, 1728, British Public Record Office Transcripts, 13: 61–67, cited in Wood, *Black Majority*, 305.

³⁶ Royal edict, October 4, 1733, SD 58–1–24, ST; Royal edict, October 29, 1733, SD 58–1–24, ST.

³⁷ Memorial of the Fugitives, SD 844, fols. 533–34, included in Manuel de Montiano to the king, March 3, 1738, SD 844, microfilm reel 15, PKY.

³⁸ Memorial of Chief Jorge, SD 844, fols. 536–37, *ibid.* Jorge claimed to be the chief who had led the Yamassee uprising against the British. Jorge stated that he and the rest of the Yamassee chiefs commonly made treaties with the slaves, and that he now wanted to help Menéndez and the three others who fought along with him become free. Mad Dog sold them into slavery for some casks of honey, corn, and liquor (*aguardiente*).

³⁹ Decree of Manuel de Montiano, March 3, 1738, SD 844, fols. 566–75, microfilm reel 15, PKY. The eight slaves who were sold to Havana included "Antonio, an English slave from San Jorge [the Spanish name for Charlestown], another of the same name, Clemente, Andres, Bartholome Chino [the term for a mixed-blood], Juan Francisco Borne, Juan (English), Jose, who's other name is Mandingo, all of whom are from San Jorge."

⁴⁰ Montiano to the king, February 16, 1739, SD 844, microfilm reel 15, PKY. The name is a composite of an existing Indian place name, Mose, the phrase that indicated that the new town was established by the king, Gracia Real, and the name of the town's patron saint, Teresa of Avilés, who was the patron saint of Spain.

all sides lined round with prickly royal and had a well and house within, and a look-out." They also confirm Spanish reports that the freedmen planted fields nearby.⁴¹ The town site was said to be surrounded by fertile lands and nearby woods that would yield building materials. A river of salt water "running through it" contained an abundance of shellfish and all types of fish.⁴² Montiano hoped the people of Mose could cultivate the land to grow food for St. Augustine, but, until crops could be harvested, he provided the people with corn, biscuits, and beef from government stores.⁴³

Mose was located at the head of Mose Creek, a tributary of the North River with access to St. Augustine, and lay directly north of St. Augustine, near trails north to San Nicholas and west to Apalache. For all these reasons, it was strategically significant. Governor Montiano surely considered the benefits of a northern outpost against anticipated British attacks. And who better to serve as an advanced warning system than grateful ex-slaves carrying Spanish arms? The freedmen understood their expected role, for, in a declaration to the king, they vowed to be "the most cruel enemies of the English" and to risk their lives and spill their "last drop of blood in defense of the Great Crown of Spain and the Holy Faith."⁴⁴ If the new homesteaders were diplomats, they were also pragmatists, and their own interests were clearly served by fighting those who would seek to return them to chattel slavery. Mose also served a vital objective of Spanish imperial policy, and, once Governor Montiano justified its establishment, the Council of the Indies and the king supported his actions.⁴⁵

Since Spanish town settlement implied the extension of *justicia*, the governor assigned a white military officer and royal official to supervise the establishment of Mose. Mose was considered a village of new converts comparable to those of the Christian Indians, so Montiano also posted a student priest at the settlement to instruct the inhabitants in doctrine and "good customs."⁴⁶ Although the Franciscan lived at Mose, there is no evidence that the white officer did. It seems rather that Captain Menéndez was responsible for governing the settlement, for, in one document, Governor Montiano referred to the others as the "subjects" of Menéndez. The Spaniards regarded Menéndez as a sort of natural lord, and, like Indian caciques, he probably exercised considerable autonomy over his village.⁴⁷ Spanish titles and support may have also reinforced Menéndez's status and authority. Whatever the nature of his authority, Menéndez commanded the Mose militia for over forty years, and his career supports Price's contention that eighteenth-century maroon leaders were military figures well-versed in European ways and equipped to negotiate their followers' best interests.⁴⁸

As new fugitives arrived, the governor placed these in Menéndez's charge as well.

⁴¹ *St. Augustine Expedition of 1740: A Report to the South Carolina General Assembly Reprinted from the Colonial Records of South Carolina with an introduction by John Tate Lanning* (Columbia, S.C., 1954), 25.

⁴² Report of Antonio de Benavides, SD 58-2-16/45, bundle 5725, ST.

⁴³ Purchases and Payments for 1739, Cuba 446, AGI.

⁴⁴ Manuel de Montiano to the king, February 16, 1739, SD 845, fol. 700, SD 845, microfilm reel 16, PKY. Fugitive Negroes of the English plantations to the king, June 10, 1738, SD 844, microfilm reel 15, PKY.

⁴⁵ Council of the Indies, October 2, 1739, cited in Wright, "Dispatches," 178-80; Council of the Indies, September 28, 1740, SD 845, fol. 708, microfilm reel 16, PKY.

⁴⁶ Manuel de Montiano to the king, February 16, 1739, SD 845, fol. 701, microfilm reel 16, PKY.

⁴⁷ Manuel de Montiano to the king, September 16, 1740, SD 2658, AGI. Montiano's successor also stated that the townspeople of Mose were "under the dominion of their Captain and Lieutenant." Melchor de Navarrete to the Marqués de Ensenada, April 2, 1752, cited in Wright, "Dispatches," 185.

⁴⁸ Evacuation report of Juan Joseph Eligio de la Puente, January 22, 1764, SD 2595, AGI; Price, *Maroon Societies*, 29-30.

A group of twenty-three men, women, and children arrived from Port Royal on November 21, 1738, and were sent to join the others at the new town. Among the newcomers were the runaway slaves of Captain Caleb Davis of Port Royal. Davis was an English merchant who had been supplying St. Augustine for many years, and it is possible that some of the runaways had even traveled to St. Augustine in the course of Davis's business. Davis went to the Spanish city in December 1738 and spotted his former slaves, whom he reported laughed at his fruitless efforts to recover them.⁴⁹ The frustrated Davis eventually submitted a claim against the Spanish for twenty-seven of his slaves "detained" by Montiano, whom he valued at 7,600 pesos, as well as for the launch in which they escaped and supplies they had taken with them. He also listed debts incurred by the citizens of St. Augustine. Among those owing him money were Governors Antonio Benavides, Francisco Moral Sánchez, and Manuel de Montiano, various royal officials and army officers, and Mose townsmen Francisco Menéndez and Pedro de Leon.⁵⁰ There is no evidence Davis ever recouped his losses.

In March 1739, envoys from Carolina arrived in St. Augustine to press for the return of their runaway slaves. Governor Montiano treated them with hospitality but referred to the royal edict of 1733, which required that he grant religious sanctuary.⁵¹ In August, an Indian ally in Apalache sent word to Montiano that the British had attempted to build a fort in the vicinity, but that the hundred black laborers had revolted, killed all the whites, and hamstringed their horses before escaping. Several days later, some of the blacks encountered the Indians in the woods and asked directions to reach the Spaniards.⁵² The following month, a group of Angola slaves revolted near Stono, South Carolina, and killed more than twenty whites before heading for St. Augustine. They were apprehended before reaching their objective, and retribution was swift and bloody. But officials of South Carolina and Georgia blamed the sanctuary available in nearby St. Augustine for the rebellion, and relations between the colonies reached a breaking point.⁵³ With the outbreak of the War of Jenkins' Ear, international and local grievances merged. In January 1740, Governor James Oglethorpe of Georgia raided Florida and captured Forts Pupo and Picolata on the St. John's River west of St. Augustine. These initial victories enabled Oglethorpe to mount a major expeditionary force, including Georgia and South Carolina regiments, a vast Indian army, and seven warships for a major offensive against the Spaniards.⁵⁴

The free black militia of Mose worked alongside the other citizenry to fortify provincial defenses. They also provided the Spaniards with critical intelligence reports.⁵⁵ In May, one of Oglethorpe's lieutenants happened across five houses

⁴⁹ Manuel de Montiano to the king, February 16, 1739, SD 845, fol. 700, microfilm reel 16, PKY; "Journal of William Stephens," cited in Wood, *Black Majority*, 307.

⁵⁰ Claim of Captain Caleb Davis, September 17, 1751, SD 2584, AGI.

⁵¹ Manuel de Montiano to the king, March 13, 1739, Manuscript 19508, Biblioteca Nacional, Madrid.

⁵² Letter of Manuel de Montiano, August 19, 1739, "Letters of Montiano, Siege of St. Augustine," *Collections of the Georgia Historical Society* (Savannah, Ga., 1909), 7: 32.

⁵³ "The Stono Rebellion and Its Consequences," in Wood, *Black Majority*, 308–26.

⁵⁴ TePaske, *Governorship*, 140.

⁵⁵ On January 8, 1740, Montiano sent Don Pedro Lamberto Horruytiner "with 25 horsemen from his company, 25 infantry and 30 Indians and free Negroes (of those who are fugitives from the English Colonies) to scout the country." Manuel de Montiano to the king, January 31, 1740, SD 2658, AGI. On January 27, 1740, Montiano sent Don Romualdo Ruiz del Moral out on a similar mission accompanied by "25 horsemen, 25 Indians, and 25 free Negroes." Montiano wrote, "The difficulty of getting information in our numerous thickets, lagoons and swamps, is so great as to make the thing almost impossible." Manuel de Montiano to the king, January 31, 1740, "Letters of Montiano," 7: 36.

occupied by the freedmen and was able to capture two of them.⁵⁶ Unable to protect the residents of Mose, Governor Montiano was forced to evacuate "all the Negroes who composed that town" to the safety of St. Augustine. Thereafter, the Mose militia continued to conduct dangerous sorties against the enemy and assisted in the surprise attack and recapture of their town in June.⁵⁷ The success at Mose was one of the few enjoyed by the Spaniards. It is generally acknowledged to have demoralized the combined British forces and to have been a significant factor in Oglethorpe's withdrawal. British accounts refer to the event as "Bloody Mose" or "Fatal Mose" and relate with horror the murder and mutilation (decapitation and castration) of two wounded prisoners who were unable to travel. They do not say whether Spaniards, Indians, or blacks did the deed. Although Spanish sources do not even mention this incident, atrocities took place on both sides. Both Spanish and British authorities routinely paid their Indian allies for enemy scalps, and at least one scalp was taken at "Moosa," according to British reports.⁵⁸

Cuban reinforcements finally relieved St. Augustine in July. Shortly thereafter, Oglethorpe and his troops returned to Georgia and Carolina.⁵⁹ Governor Montiano commended all his troops to the king but made the rather unusual gesture of writing a special recommendation for Francisco Menéndez. Montiano extolled the exactitude with which Menéndez had carried out royal service and the valor he had displayed in the battle at Mose. He added that, on another occasion, Menéndez and his men had fired on the enemy until they withdrew from the castle walls and that Menéndez had displayed great zeal during the dangerous reconnaissance missions he undertook against the British and their Indians. Moreover, he acknowledged that Menéndez had "distinguished himself in the establishment, and cultivation of Mose, to improve that settlement, doing all he could so that the rest of his subjects, following his example, would apply themselves to work and learn good customs."⁶⁰

Shortly thereafter, Menéndez petitioned for remuneration from his king for the "loyalty, zeal and love I have always demonstrated in the royal service, in the encounters with the enemies, as well as in the effort and care with which I have worked to repair two bastions on the defense line of this plaza, being pleased to do it, although it advanced my poverty, and I have been continually at arms, and assisted in the maintenance of the bastions, without the least royal expense, despite the scarcity in which this presidio always exists, especially in this occasion." He added, "my sole object was to defend the Holy Evangel and sovereignty of the Crown," and asked for the proprietorship of the free black militia and a salary to enable him to live decently (meaning in the style customary for an official of the militia). He concluded that he hoped to receive "all the consolation of the royal support . . . which Christianity requires and your vassals desire." Several months

⁵⁶ *St. Augustine Expedition*, 23. One was the escaped slave of Mrs. Parker, and the other claimed to have been carried away from Colonel Gibbs by the Indians.

⁵⁷ Manuel de Montiano to the king, January 17, 1740, SD 2658, AGI. For Montiano's account of Oglethorpe's siege and the victory at Mose, see Manuel de Montiano to the king, August 9, 1740, SD 845, fols. 11–26, microfilm reel 16, PKY; and "Letters of Montiano," 7: 54–62.

⁵⁸ Mills Lane, ed. *General Oglethorpe's Georgia: Colonial Letters, 1738–1743*, II (Savannah), 447. For more accounts of atrocities, see *St. Augustine Expedition of 1740*, 47; TePaske, *Governorship of Spanish Florida*, 143; Larry E. Ivers, *British Drums on the Southern Frontier, The Military Colonization of Georgia 1733–1749* (Chapel Hill, N.C., 1974). This account is written from British sources and therefore is inaccurate on many aspects of the Spanish history. Ivers seriously undercounts St. Augustine's population, glamorizes Oglethorpe's role, and fails to recognize the role of blacks at Mose and throughout the Anglo-Spanish conflict.

⁵⁹ TePaske, *Governorship of Spanish Florida*, 144.

⁶⁰ Manuel de Montiano to the king, January 31, 1740, SD 2658, AGI.

later, Menéndez filed a second, shorter petition.⁶¹ It was customary for an illiterate person to sign official documents with an X, and for the notary or witnesses to write underneath, “for ———, who does not know how to write.” Both these petitions, however, were written and signed in the same hand and with a flourish, so it would seem that at some point Menéndez learned how to write in Spanish—perhaps when he was the slave of the royal accountant whose name he took.⁶² Despite his good services, appropriate behavior and rhetoric, there is no evidence of a response, and the noted royal parsimony made such payment unlikely.

Nevertheless, the runaways from Carolina had been successful in their most important appeal to Spanish justice—their quest for liberty. Over the many years, they persevered, and their leaders learned to use Spanish legal channels and social systems to advantage. They accurately assessed Spain’s intensifying competition with England and exploited the political leverage it offered them. Once free, they understood and adapted to Spanish expectations of their new status. They vowed fealty and armed service, establishing themselves as vassals of the king and deserving of royal protection. Governor Montiano commended their bravery in battle and their industry as they worked to establish and cultivate Mose. They were clearly not the lazy vagabonds feared by Spanish administrators, and the adaptive behavior of Menéndez and his “subjects” gained them at least a limited autonomy.

Such autonomy is evident in both the black and Indian militias that operated on St. Augustine’s frontiers. Their role in the defense of the Spanish colony has not yet been appreciated. They were cavalry units that served in frontier reconnaissance and as guerrilla fighters. They had their own officers and patrolled independently, although Spanish infantry officers also commanded mixed groups of Spanish, free blacks, and Indians on scouting missions.⁶³ The Florida garrison was never able to maintain a full contingent, and these militias constituted an important asset for the short-handed governors.⁶⁴ Because England and Spain were so often at war during his administration, Governor Montiano probably depended on the black troops more than did subsequent Florida governors.

When the Spaniards mounted a major retaliatory offensive against Georgia in 1742, Governor Montiano once again employed his Mose militia. Montiano’s war plans called for sending English-speaking blacks of the Mose militia to range the countryside gathering and arming slave recruits, which suggests that he placed great trust in their loyalty and ability, as well, perhaps, as in their desire to punish their former masters.⁶⁵ Bad weather, mishaps, and confusion plagued the operation, and several hundred of the Spanish forces were killed at Bloody Marsh on Saint Simon’s island. By August, the Spaniards had returned to St. Augustine.

⁶¹ Memorial of Francisco Menéndez, November 21, 1740, SD 2658, AGI. Memorial of Francisco Menéndez, December 12, 1740, *ibid.*

⁶² The proprietary royal accountant for St. Augustine was Don Francisco Menéndez Márquez. The Menéndez Márquez family is the subject of several works by Amy Turner Bushnell. See “The Menéndez Márquez Cattle Barony at La Chua and the Determinants of Economic Expansion in Seventeenth-Century Florida,” *Florida Historical Quarterly*, 56 (April 1978): 407–31; and *The King’s Coffers, Proprietors of the Spanish Florida Treasury, 1565–1702* (Gainesville, Fla., 1981).

⁶³ Manuel de Montiano to the king, January 22, 1740, “Letters of Montiano,” 7: 32–42. Indian militias continued to serve Florida’s governors, and in 1759 Cacique Bernardo Lachiche commanded a unit of twenty-eight men, by election of the other caciques. Report of Don Lucas de Palacio on the Spanish, Indian and Free Black Militias, April 30, 1759, SD 2604, AGI.

⁶⁴ Although St. Augustine was allotted a troop complement of 350 men, Montiano had only 240 men fit for service in St. Augustine when the siege of 1740 ended. Manuel de Montiano to the king, August 9, 1740, SD 846, fol. 25 V, microfilm reel 16, PKY.

⁶⁵ Manuel de Montiano to the captain general of Cuba, Don Juan Francisco de Güemes y Horcasitas, March 13, 1742, SD 2593, AGI; TePaske, *Governorship of Spanish Florida*, 146–52.

Oglethorpe mounted two more attacks on St. Augustine in 1742 and 1743, but neither did major damage. An uneasy stalemate developed, punctuated occasionally by Indian and corsair raids.⁶⁶

Corsairing was practiced by both the British and the Spanish during the 1740s and 1750s, and St. Augustine became a convenient base of operations for privateers commissioned by Spain. The capture and sale of prizes provided badly needed species and supplies for war-torn Florida, which had not received government subsidies in 1739, 1740, 1741, and 1745 and which struggled under the additional burden of maintaining the large number of Cuban reinforcements that had arrived in 1740.⁶⁷ Corsairing ships were manned by volunteers, some of whom were drawn from the free black community, for, as Governor García noted, "without those of 'broken' color, blacks, and Indians, which abound in our towns in America, I do not know if we could arm a single corsair solely with Spaniards."⁶⁸ Unfortunately, when these men were captured, the British presupposed them by their color to be slaves and sold them for profit.

When the British ship *Revenge* captured a Spanish prize in July 1741, found aboard was a black named "Signior Capitano Francisco," who was "Capt. of a Comp'y of Indians, Mollattos, and Negroes that was att the Retaking of the Fort [Mose] att St. Augus'ne formerly taken Under the Command of that worthless G—— O——pe who by his treachory suffered so many brave fellows to be mangled by those barbarians." His captors tied Francisco Menéndez to a gun and ordered the ship's doctor to pretend to castrate him (as Englishmen at Mose had been castrated), but while Menéndez "frankly owned" that he was Captain of the company that retook Mose, he denied ordering any atrocities, which he said the Florida Indians had committed. Menéndez stated that he had taken the commission as privateer in hopes of getting to Havana, and from there to Spain, to collect a reward for his bravery. Several other mulattoes on board were also interrogated and substantiated Menéndez's account, as did several of the whites, but "to make Sure and to make him remember that he bore such a Commission," the British gave him 200 lashes and then "pickled him and left him to the Doctor to take Care of his Sore A—se." The following month, the *Revenge* landed at New Providence, in the Bahamas, and her commander, Benjamin Norton, who was due the largest share of the prize, vehemently argued before the Admiralty Court that the blacks should be condemned as slaves. "Does not their Complexion and features tell all the world that they are of the blood of Negroes and have suckt Slavery and Cruelty from their Infancy?" He went on to describe Menéndez as "this Francisco that Cursed Seed of Cain, Curst from the foundation of the world, who has the Impudence to Come into this Court and plead that he is free. Slavery is too Good for such a Savage, nay all the Cruelty invented by man . . . the torments of the World to Come will not suffice." No record of Francisco's testimony appears in this account, but the Court ordered him sold as a slave, "according to the Laws of the plantation."⁶⁹ However, as we have seen, Menéndez was a man of unusual abilities. Whether he successfully appealed for his freedom in British courts as he had in the Spanish, was ransomed back by the Spanish in Florida, or escaped is unknown, but, by at least 1752, he was

⁶⁶ TePaske, *Governorship of Spanish Florida*, 152–55.

⁶⁷ TePaske, *Governorship of Spanish Florida*, 100–05.

⁶⁸ Fulgencio García de Solís to the king, August 25, 1752, SD 845, fols. 81–112, microfilm reel 17, PKY.

⁶⁹ "Account of the Revenge," in John Franklin Jameson, ed., *Privateering and Piracy in the Colonial Period: Illustrative Documents* (New York, 1923), 402–11. My thanks to Charles Tingley for providing this source.

once again in command at Mose. This incident illustrates the extreme racial hatred some British felt for Spain's black allies, as well as the grave dangers the freedmen faced in taking up Spanish arms. Other blacks captured as privateers in the same period were never returned.⁷⁰

ALTHOUGH UNSUCCESSFUL, GOVERNOR OGLETHORPE's invasion in 1740 had wreaked havoc in Spanish Florida. Mose and the other outlying forts had been destroyed, along with many of the crops and animals on which the community subsisted. For the next twelve years, the townspeople of Mose lived among the Spanish in St. Augustine. This interlude was critical to the integration of the Carolina group into the larger and more diverse society in the city. Wage lists in treasury accounts and military reports from this period show that they performed a variety of valuable functions for the community. Free blacks labored on government projects, were sailors and privateers, tracked escaped prisoners, and helped forage food for the city. In the spring, they rounded up wild cattle for slaughter and wild horses for cavalry mounts.⁷¹ They probably led lives much like those of free blacks in other Spanish colonial ports and may have engaged in craft production, artisanry, and the provision of services.⁷² Although certain racial restrictions existed, they were rarely enforced in a small frontier settlement such as St. Augustine, where more relaxed personal relations were the norm. Everyone knew everyone else, and this familiarity could be a source of assistance and protection for the free blacks of Mose, who had acquired at least a measure of acceptability.⁷³

Parish registers reflect the great ethnic and racial diversity in Spanish Florida in these years. Because there were always fewer female runaways, the males of that group were forced to look to the local possibilities for marriage partners—either Indian women from the two outlying villages of Nuestra Señora de la Leche and Nuestra Señora de Tholomato, or free and slave women from St. Augustine. Interracial relationships were common, and families were restructured frequently when death struck and widowed men and women remarried. The core group of Carolina fugitives formed intricate ties among themselves for at least two generations. They married from within their group and served as witnesses at each other's weddings and as godparents for each other's children, sometimes many times over. They also entered into the same relationships with Indians, free blacks, and slaves from other locations. Some of these slaves eventually became free, which might suggest mutual assistance efforts by the black community. The people of Mose also

⁷⁰ Report of Captain Fernando Laguna, October 7, 1752, SD 846, fols. 84–108, microfilm reel 17, PKY.

⁷¹ Michael C. Scardaville and Jesus María Belmonte, "Florida in the Late First Spanish Period: The Griñán Report," *El Escribano*, 16 (1979): 10.

⁷² Works that provide information on the life and labor of blacks in colonial Spanish America include: Jorge Juan and Antonio de Ulloa, "Eighteenth-Century Spanish American Towns—African and Afro-Hispanic Life and Labor in Cities and Suburbs," in Anne Pescatello, ed., *The African in Latin America* (New York, 1975): 106–11; Greene and Cohen, *Neither Slave nor Free*; and Louisa Schell Hoberman and Susan Migden Socolow, eds., *Cities and Society in Colonial America* (Albuquerque, N. Mex., 1986).

⁷³ Michael P. Johnson and James L. Roark's work, *Black Masters: A Free Family of Color in the Old South* (New York, 1984), demonstrates how personalism might mediate race relations even in a more rigid caste society, but free blacks always had to balance carefully their legal rights against the social limits accepted in their community. For other examples of upwardly mobile slaves from Spanish Florida, see Jane Landers, "Black Society in Spanish St. Augustine, 1784–1821" (Ph.D. dissertation, University of Florida, Gainesville, 1988).

formed ties of reciprocal obligation with important members of both the white and black communities through the mechanism of ritual brotherhood (*compadrazgo*). A few examples should serve to illustrate the complex nature of these frontier relationships.

Francisco Garzía and his wife, Ana, fled together from Carolina and were among the original group freed by Governor Montiano. Francisco was black, and Ana, Indian. As slaves in St. Augustine, they had belonged to the royal treasurer, Don Salvador Garzía. Garzía observed the church requirement to have his slaves baptized and properly married, for the couple's children are listed as legitimate. Francisco and Ana's daughter, Francisca Xavier, was born and baptized in St. Augustine in 1736, before her parents were freed by the governor. Her godfather was a free mulatto, Francisco Rexidor. This man also served as godfather for Francisco and Ana's son, Calisto, born free two years later. Garzía died sometime before 1759, for in that year his widow, Ana, married a black slave named Diego. Calisto disappeared from the record and presumably died, while Francisca Xavier married Francisco Díaz, a free black from Carolina. Their two children, Miguel Francisco and María, were born at Mose, and Francisco Díaz served in the Mose militia.⁷⁴

Juan Jacinto Rodríguez and his wife, Ana María Menéndez, were also among the first Carolina homesteaders at Mose. Shortly after the town was founded, their son Juan married Cecilia, a Mandingo from Carolina who was the slave of Juan's former owner, cavalry Captain Don Pedro Lamberto Horruytiner. Cecilia's sister-in-law, María Francisca, had served as godmother at Cecilia's baptism two years earlier. María Francisca married Marcos de Torres, a free and legitimate black from Cartagena, Colombia, during the time the Mose homesteaders lived in St. Augustine. Marcos de Torres and María Francisca had three children born while they lived in town, and María Francisca's brother, Juan, and his wife, Cecilia, served as the children's godparents. After Marcos de Torres died, María Francisca and her three orphaned children lived with her parents at Mose. In 1760, the widowed María Francisca married the widower, Thomas Chrisostomo.⁷⁵

Thomas and his first wife were Congo slaves. Thomas belonged to Don Francisco Chrisostomo, and his wife, Ana María Ronquillo, to Juan Nicolás Ronquillo. The couple married in St. Augustine in 1745. Pedro Graxales, a Congo slave and his legitimate wife, María de la Concepción Hita, a Caravalí slave, were the godparents at the wedding. By 1759, Thomas was a free widower living alone at Mose. The

⁷⁴ In 1738, Francisco and Ana were the slaves of Don Salvador Garzía, SD 844, fols. 593–94, microfilm reel 15, PKY. Baptism of Francisca Xavier, August 30, 1736, and baptism of Calisto, October 23, 1738, Black Baptisms, Cathedral Parish Records, Diocese of St. Augustine Catholic Center, Jacksonville (hereafter cited as CPR), microfilm reel 284 F, PKY. Marriage of the widowed Ana García Pedroso to Diego, the slave of Don Juan Joseph Eligio de la Puente, January 14, 1759, Black Marriages, CPR, microfilm reel 284 C, PKY. Baptism of Miguel Francisco, January 29, 1753, Black Baptisms, CPR, microfilm reel 284 F, PKY. Mose militia list, included in evacuation report of Juan Joseph Eligio de la Puente, January 22, 1764, SD 2595, AGI.

⁷⁵ In 1738, Juan Jacinto Rodríguez and Ana María Menéndez were the slaves of Petronila Pérez, SD 844, fol. 594, PKY. They were married as slaves on October 9, 1735, CPR, 284 C, PKY. After Juan Jacinto died, Ana María married the free black, Antonio de Urisa, of the Lara nation on April 26, 1740, *ibid.* Juan Jacinto and Ana María's daughter, María Francisca, was baptized on October 11, 1736, while she was still the slave of Petronila Pérez, CPR, 284 F, PKY. Juan Lamberto Horruytiner married Cecilia Horruytiner on July 12, 1739, CPR, 284 C, PKY. Baptism of Cecilia, September 9, 1737, CPR, 284 F, PKY. Marriage of María Francisca to Marcos de Torres, August 20, 1742, CPR, 284 C, PKY. Baptism of their daughter, María, May 20, 1743, and their son Nicolás de la Concepción, January 10, 1746, CPR, 284 F, PKY. María Francisca and the children were living in her parents' home at the time of the 1759 census. Census of Father Gines Sánchez, February 12, 1759, SD 2604, AGI. Marriage of Thomas Chrisostomo and María Francisca, December 15, 1760, CPR, 284 C, PKY.

next year, he and María Francisca were wed. By that time, Thomas's godfather, Pedro Graxales, was also living at Mose as a free man, but Pedro's wife and at least four children remained slaves in St. Augustine.⁷⁶

A simple bicultural encounter model will not suffice to explain the extent of cultural adaptation at Mose and the formation of this African-Hispanic community.⁷⁷ Many of its members were born on the western coast of Africa and then spent at least some time in a British slave society before risking their lives to escape. Some had intimate contact for several years with the Yamassee Indians and fought other non-Christian Indian groups before reaching Spanish Florida. At least thirty-one became slaves of the Spanish prior to achieving free status. Once free, they associated closely with the remnants of the seven different Indian nations aggregated into the two outlying Indian towns. From 1740 until 1752, the Mose group lived within the city of St. Augustine; after that time, they were forcibly removed to a rebuilt settlement. Meanwhile, new infusions of Africans continued to be incorporated into the original Mose community through ties with godparents. Many historians now agree that, although the ex-slaves did not share a single culture, their common values and experiences in the Americas enabled them to form strong communities, as they did in Spanish Florida. Ira Berlin, Steven F. Miller, and Leslie S. Rowland have argued that British slaves understood their society "in the idiom of kinship" and that, for slaves, "familial and communal relations were one."⁷⁸ The Spaniards also viewed society as an extension of family structures. The institution of the extended kinship group (*parentela*), which included blood relations, fictive kin, and sometimes even household servants and slaves, and the institution of *clientela*, which bound powerful patrons and their personal dependents into a network of mutual obligations, were so deeply rooted in Spain that, according to one scholar, they might have been the "primary structure of Hispanic society." Thus African and Spanish views of family and society were highly compatible, and each group surely recognized the value that the other placed on kinship.⁷⁹

Despite the relationships that developed between people of St. Augustine and the Mose settlers, there were objections to their presence in the Spanish city. Some complaints may have stemmed from racial prejudice or ethnocentrism. To some of the poorer Spanish, the free blacks represented competition in a ravaged economy. Indians allied to the British remained hostile to the Spaniards and raided the countryside with regularity. Plantations were neither safe nor productive. Havana could not provide its dependency with sufficient goods, and the few food shipments that reached St. Augustine were usually ruined. British goods were cheaper and better, and the governor was forced to depend on enemy suppliers for his needs. War and corsair raids on supplies shipped from Havana further strained the colony's ability to sustain its urban population. As new runaways continued to

⁷⁶ Marriage of Thomas Chrisostomo and Ana María Ronquillo, February 28, 1745, *ibid.* Baptism of Pedro Graxales, December 9, 1738, CPR, 284 F, PKY. Marriage of Pedro Graxales, Congo slave of Don Francisco Graxales, and María de la Concepción Hita, Caravali slave of Don Pedro de Hita, January 19, 1744, CPR, 284 C, PKY. Baptisms of their children, María, November 4, 1744; Manuela de los Angeles, January 1, 1747; Ysidora de los Angeles, December 22, 1748; Joseph Ynisario, April 4, 1755; and Juana Feliciania, July 13, 1757, CPR, 284 F, PKY.

⁷⁷ Mintz and Price, "Anthropological Approach."

⁷⁸ Licenses for Slaves Imported into St. Augustine, 1762–1763, Cuba 472, AGI. Ira Berlin, Steven F. Miller, and Leslie S. Rowland, "Afro-American Families in the Transition from Slavery to Freedom," *Radical History Review*, 42 (1988): 89.

⁷⁹ For a concise description of the importance of the extended family, *parentela*, and the system of personal dependency, *clientela*, in Spain, see McAlister, *Spain and Portugal*, 39–40.

arrive, they only exacerbated the problem.⁸⁰ Finally, Melchor de Navarrete, who succeeded Montiano in 1749, decided to reestablish Mose. He reported his achievements in converting the newcomers, remarking that he withheld certificates of freedom until the supplicants had a satisfactory knowledge of doctrine. Navarrete also claimed to have resettled all the free blacks from Carolina at Mose.⁸¹

Governor Fulgencio García de Solís, who served from 1752 to 1755, refuted his predecessor's claims, stating that persistent illnesses among the blacks had prevented their relocation. When García attempted to remove the freedmen and women to Mose, he faced stubborn resistance. The governor complained that it was not fear of further Indian attacks but the "desire to live in complete liberty" that motivated the rebels. He "lightly" punished the two unnamed leaders of the resistance and threatened worse to those who continued to fight the resettlement. He fortified the town to allay their fears and finally effected the resettlement. In a familiar litany, he alluded to "bad customs," "spiritual backwardness," and "pernicious consequences" and condemned not only the original Mose settlers but also "those who have since fled the English colonies to join them." He was determined that they would have "no pretext which could excuse them" from living at Mose and sought to isolate them from "any dealings or communication with . . . the town within the walls."⁸² The Spanish association of urbanization with the advance of civilization traditionally had as its corollary the idea that those living outside a city's boundaries were lacking in cultural and spiritual attainments. In his official papers, García evidenced a much lower opinion of the free blacks than had Governor Montiano, and by removing them "beyond the walls" he made a visible statement about their supposed inferiority.

García was no doubt angered by the rebellion he faced, and he was probably correct in contending that it actually arose from the free black desire to live in "complete liberty." The crown had many times reiterated its commitment to their freedom, and, after living in St. Augustine for thirteen years and repeatedly risking their lives in its defense, the free blacks surely recognized the eviction for the insult it represented. Possibly, after García's interim term ended, there was greater interaction between the peoples of St. Augustine and its satellite, as later governors did not display his antipathy toward the free blacks.⁸³ Governor García may also have been disturbed by the presence and influence of unacculturated Africans (*bozales*) among the latecomers. The "bad customs" that he alleged had so troubled his predecessors and himself might have been African cultural retentions. In 1744, Father Francisco Xavier Arturo baptized Domingo, a Caravalí slave, in extremis, with the comment that his "crudeness" prevented his understanding Christian doctrine.⁸⁴ Four years later, Miguel Domingo, a Congo slave, received a conditional

⁸⁰ TePaske, *Governorship*, 227–29. TePaske described the chronic financial shortages of Florida, saying that "poverty and want characterized life in Florida and pervaded all aspects of life." Father Juan Joseph de Solana also described Florida as a destitute colony, impoverished, despite its natural resources, by the continual attack of Indians loyal to the British. Father Juan Joseph de Solana to Bishop Pedro Agustín Morel de Sánchez, April 22, 1759, SD 516, microfilm reel 28 K, PKY.

⁸¹ Melchor de Navarrete to the Marqués de Ensenada, April 2, 1752, cited in Wright, "Dispatches," 184–86.

⁸² Fulgencio García de Solís to the king, November 29, 1752, SD, microfilm reel 17, PKY. Also, Fulgencio García de Solís to the king, December 7, 1752, cited in Wright, "Dispatches," 187–89.

⁸³ Governors Alonso Fernández de Heredia and Lucas de Palacio both requested special financial assistance for the townspeople of Mose, citing their poverty. Alonso de Heredia to Julian de Arriaga, April 7, 1756, cited in Wright, "Dispatches," 193–94; the king to Lucas de Palacio, April 21, 1759, *ibid.*, 195.

⁸⁴ Baptism of Miguel, 1744, CPR, 284 F, PKY.

baptism, because he told the priest that he had been baptized in his homeland, and continued to pray in his native language.⁸⁵

Peter Wood's analysis of slave imports into South Carolina during the late 1730s determined that 70 percent of those arriving during this brief period came from the Congo-Angola region.⁸⁶ St. Augustine's church registers suggest a similar preponderance there but within a broader context of considerable ethnic diversity. The Spanish often recorded the nation of origin for the Africans among them, and, although these designations are troublesome and must be used with caution, they offer at least a general approximation of the origins of those recorded. One hundred and forty-seven black marriages were reported from 1735 to 1763, and fifty-two of those married were designated as Congos—twenty-six males and twenty-six females. The next largest group was the Caravallís, including nine males and nineteen females. The Mandingos constituted the third largest group and had nine males and four females. Also represented in the marriage registers were the Minas, Gambas, Lecumis, Sambas, Gangas, Araras, and Guineans.⁸⁷

Governor García was required by royal policy to grant sanctuary to slave refugees, but he was not required to accommodate them in St. Augustine, and he did not. The chastened freedmen built new structures at Mose, including a church and a house for the Franciscan priest within the enclosed fort, as well as twenty-two shelters outside the fort for their own households. A diagram of the new fort, which had one side open on Mose Creek, shows the interior buildings described by Father Juan Joseph de Solana but not the houses of the villagers.⁸⁸ The only known census of Mose, from 1759, recorded twenty-two households with a population of sixty-seven individuals. Mose had almost twice as many male as female occupants, and almost a quarter of its population consisted of children under the age of fifteen. Thirteen of the twenty-two households belonged to nuclear or nuclear extended families, and fifty villagers, or 75 percent of the total population, lived with immediate members of their families. There were no female-headed households at this outpost, and nine households were composed solely of males. At the time of the census, four men lived alone, Francisco Roso, Antonio Caravalló, Thomas Chrisostomo, and Antonio Blanco, but at least two of those men, Roso and Chrisostomo, had family members among the slaves in St. Augustine. A third all-male household consisted of a father, Francisco de Torres, and his son, Juan de Arranzate. Francisco's wife and Juan's mother, Ana María, was a slave in St. Augustine. Pedro Graxales was also separated from his slave wife and their children but had a younger man, Manuel Rivera, attached to his household. Three other all-male households included a total of eleven men living together, at least three of whom had slave wives in St. Augustine.⁸⁹ Although spouses lived

⁸⁵ Baptism of Miguel Domingo, January 26, 1748, CPR, 284 F, PKY.

⁸⁶ Wood, *Black Majority*, 302.

⁸⁷ Dr. Kathleen Deagan, Florida Museum of Natural History, University of Florida, Gainesville, provided these figures.

⁸⁸ In a map drawn by Pablo Castello, 1763, 833 B, PKY.

⁸⁹ Census of Gines Sánchez, February 12, 1759, SD 2604, AGI. Marriage of Francisco Roso, free Caravalló and María de la Cruz, Caravalló slave of Don Carlos Frison, January 8, 1743, CPR, 284 C, PKY; Baptism of Carlos Roso, November 4, 1743, CPR, 284 F, PKY. Marriage of Francisco Xavier de Torres, Mandingo, to Ana María, Mandinga slave of Josepha de Torres, February 1, 1752, CPR, 284 C, PKY. Others with slave wives in St. Augustine were Joseph de Peña, Caravalló, married to Ana María Ysquierdo, Conga slave of Don Juan Ysquierdo, January 29, 1743, *ibid.*; Juan Francisco de Torres, married to María Guillen slave of Joseph Guillen, January 21, 1743, *ibid.*; Joseph Fernández, Mandingo, married to Ana María, Caravalló slave, December 1, 1756, *ibid.*; Juan Baptista married to María de Jesus, August 17, 1757, *ibid.*

separately, parish registers record that children continued to be born of these unions and attest that family ties were maintained. Father Solana reported that some members of the Mose community were permitted to live in St. Augustine even though they continued to serve in the Mose militia. Several of those men appear on 1763 evacuation lists for Mose.⁹⁰

THE PEOPLE OF MOSE WERE REMARKABLY ADAPTABLE. They spoke several European and Indian languages, in addition to their own, and were exposed to a variety of subsistence techniques, craft and artistic traditions, labor patterns, and food ways. We know that the freedmen and women of Mose adopted certain elements of Spanish culture. For example, since their sanctuary was based on religious conversion, it was incumbent on them to exhibit their Catholicism. Their baptisms, marriages, and deaths were faithfully recorded in parish registers. But studies of other Hispanic colonies show that religious syncretism was widespread and tolerated by the church. Following centuries-old patterns set in Spain, Cuba's blacks organized religious brotherhoods by nations. They celebrated Catholic feast days dressed in traditional African costumes and with African music and instruments.⁹¹ Because St. Augustine had such intimate contact with Cuba and blacks circulated between the two locations, it would not be surprising to find that Africans in Florida also observed some of their former religious practices.

Kathleen Deagan, of the Florida Museum of Natural History, currently directs an interdisciplinary team investigating Mose. In addition to locating and excavating the site, this group is exploring the process of cultural adaptation at Mose to determine what mixture of customs and material culture its residents adopted and what in their own traditions might have influenced Spanish culture.⁹² One suggestive find is a hand-made pewter medal that depicts St. Christopher on one side and a pattern resembling a Kongo star on the other.⁹³ Other recovered artifacts include military objects such as gunflints, a striker, and musket balls; and domestic articles such as metal buckles, a thimble, and pins, clay pipe bowls—of both local and European design—metal buttons, bone buttons—including one still in the process of manufacture—amber beads (perhaps from a rosary); and a variety of glass bottles and ceramic wares. Many of the latter are of English types, verifying documentary evidence of illicit, but necessary, trade with the enemy.

Preliminary analysis of faunal materials from the site indicates that the diet at Mose approximated that of indigenous villages and supports documentary evidence that the Indian and black villages resembled each other in many respects.

⁹⁰ Report of Father Juan Joseph de Solana to Bishop Pedro Agustin Morel de Sánchez, April 22, 1759, SD 516, microfilm reel 28 K, PKY; Evacuation report of Juan Joseph Eligio de la Puente, January 22, 1764, SD 2595, AGI.

⁹¹ Fernando Ortiz, "La Fiesta Afro-Cubana del 'Día de Reyes,'" *Revista Bimestre Cubana*, 15 (January–June 1920): 5–16.

⁹² Kathleen Deagan analyzes elements and patterns of cultural exchange and adaptation in several works. See *Artifacts of the Spanish Colonies of Florida and the Caribbean, 1500–1800* (Washington, D.C., 1987); *Spanish St. Augustine: The Archaeology of a Colonial Creole Community* (New York, 1983); *St. Augustine: First Urban Enclave in the United States* (Farmingdale, N.Y., 1982); and *Sex, Status and Role in the Mestizaje of Spanish Colonial Florida* (Gainesville, Fla., 1974). On African-American archaeology, see Theresa Singleton, *The Archaeology of Slavery and Plantation Life* (Orlando, Fla., 1985); and Leland Ferguson, "Looking for the 'Afro' in Colono-Indian Pottery," in Robert L. Schuyler, ed., *Archaeological Perspectives on Ethnicity in America: Afro-American and Asian American Culture History* (Farmingdale, N.Y., 1980), 14–28.

⁹³ On Kongo-American connections, see Thompson, *Flash of the Spirit*, 112–15.

Mose's villagers incorporated many estuarine resources and wild foods into their diet. The fish were net-caught, perhaps using African techniques. The people at Mose also caught and consumed deer, raccoon, opossum, and turtle to supplement the corn and beef occasionally provided them from government stores.⁹⁴

Although noted for its poverty and the misery of its people, Mose survived as a free town and military outpost for St. Augustine until 1763, when, through the fortunes of war, Spain lost the province to the British. The Spanish evacuated St. Augustine and its dependent black and Indian towns, and the occupants were resettled in Cuba. The people of Mose left behind their meager homes and belongings and followed their hosts into exile to become homesteaders in Matanzas, Cuba—consigned once more to a rough frontier. The crown granted them new lands, a few tools, and a minimal subsidy, as well as an African slave to each of the leaders of the community; however, Spanish support was never sufficient, and the people from Mose suffered terrible privations at Matanzas. Some of them, including Francisco Menéndez, eventually relocated in Havana, which offered at least the possibility of a better life, and this last diaspora scattered the black community of Mose.⁹⁵

Located on the periphery of St. Augustine, between the Spanish settlement and its aggressive neighbors, Mose's interstitial location paralleled the social position of its inhabitants—people who straddled cultures, pursued their own advantage, and in the process helped shape the colonial history of the Caribbean as well as an African-American culture. In 1784, Spain recovered Florida, and many Floridanos, or first-period colonists, returned from Cuba. It is possible that among these were some of the residents of Mose. During its second regime, however, the weakened Spanish government made no effort to reestablish either Indian missions or the free black town of Mose. Free blacks took pivotal roles on interethnic frontiers of Spanish America such as Florida, serving as interpreters, craftsmen, traders, scouts, cowboys, pilots, and militiamen. The towns they established made important contributions to Spanish settlement. They populated areas the Spaniards found too difficult or unpleasant, thereby extending or maintaining Spanish dominion. They buffered Spanish towns from the attacks of their enemies and provided them with effective military reserves.

Although there were other towns like Mose in Latin America, it was the only example of a free black town in the colonial South. It provides an important, and heretofore unstudied, variant in the experience of African-born peoples in what was to become the United States. Mose's inhabitants were able to parlay their initiative, determination, and military and economic skills into free status, an autonomy at least equivalent to that of Spain's Indian allies in Florida, and a town of their own. These gains were partially offset by the constant danger and deprivation to which the townspeople of Mose were subjected, but they remained in Mose, perhaps believing it their best possible option. Despite the adversities of slavery, flight, wars, and repeated displacements, the freedmen and women of Mose managed to maintain intricate family relationships over time and shape a viable community under extremely difficult conditions. They became an example

⁹⁴ Personal communication from Kathleen Deagan, October 1989. On African fishing techniques in the colonial southeast, see Peter H. Wood, "It Was a Negro Taught Them': A New Look at African Labor in Early South Carolina," *Journal of Asian and African Studies*, 9 (July and October 1974): 167–68.

⁹⁵ Evacuation Report of Juan Joseph Eligio de la Puente, January 22, 1764, SD 2595, AGI. Accounts of the royal treasury of Matanzas, 1761–82, SD 1882, AGI. At least one family attempted to recover the losses of the evacuation, but they were denied on the basis of their color. Petition of María Gertrudis Roso, September 25, 1792, SD 2577, AGI.

and possibly a source of assistance to unfree blacks from neighboring British colonies, as well as those within Spanish Florida. The Spanish subsequently extended the religious sanctuary policy confirmed at Mose to other areas of the Caribbean and applied it to the disadvantage of Dutch and French slaveholders, as well as the British.⁹⁶ The lives and efforts of the people of Mose thus took on international significance. Moreover, their accomplishments outlived them. The second Spanish government recognized religious sanctuary from 1784 until it bowed to the pressures of the new U.S. government and its persuasive secretary of state, Thomas Jefferson, and abrogated the policy in 1790. Before that escape hatch closed, several hundred slaves belonging to British Loyalists followed the example of the people of Mose to achieve emancipation in Florida.⁹⁷ Thus the determined fugitives who struggled so hard to win their own freedom inadvertently furthered the cause of freedom for others whom they never knew.

⁹⁶ Slaves escaped from Guadaloupe to Puerto Rico in 1752, and the case was still before the Council of the Indies twenty years later; Consulta, July 19, 1772. Slaves from the Danish colonies of Santa Cruz and Santo Thomas also fled to Puerto Rico in 1767, and eventually the governments signed a convention; Consulta, July 21, 1777. Slaves from the Dutch settlement at Esquibo fled to Guyana, October 22, 1802; Documents relating to fugitive slaves, Indiferente General 2787, AGI.

⁹⁷ See Landers, "Spanish Sanctuary." Upon registering themselves and obtaining work contracts, slaves escaped from British colonists were freed by the second Spanish government; Census Returns, 1784-1814, East Florida Papers, PKY, microfilm reel 323 A; Royal decree, included in Captain General Luis de las Casas to Governor Manuel Vicente de Zéspedes, July 21, 1790, East Florida Papers, PKY, microfilm reel 1.

Prosperous Blacks in the South, 1790–1880

LOREN SCHWENINGER

"THERE ARE ALSO, IN THE VICINITY, a large number of free-colored planters," Frederick Law Olmsted wrote in 1856, a few years after a steamboat trip down the Cane River in Louisiana. Having stopped at several plantations to take on cotton, he had learned that, in fifteen miles of "well-settled and cultivated country" on the bank of the river, beginning ten miles below Natchitoches, there was only "one pure blooded white man." Describing these planters as "GALLIC AND HISPANO-AFRIC CREOLES," Olmsted noted that they were the slaveholding descendants of "old French or Spanish planters and their negro slaves." As a reporter for the *New York Times* and *New York Tribune*, Olmsted had traveled extensively in the South during the early 1850s, publishing four books about his experiences, but few groups stood out more vividly in his memory than Louisiana's Creoles of color. Not only did these American-born people of color possess a European and African cultural heritage but they also acquired substantial wealth and property.¹

For many years, historians paid only slight attention to blacks who reached the upper economic levels in the nineteenth-century South. In 1905, amateur historian Calvin Dill Wilson wrote a ten-page essay in the *North American Review* called "Black Masters: A Side-Light on Slavery," and a decade later John Russell added a brief article in the *Journal of Negro History* on the same subject.² The "scientific historians" of the William A. Dunning school—Walter Lynwood Fleming, Mildred Thompson, James G. De Roulhac Hamilton, James W. Garner, among others—almost completely ignored black landholders and prosperous black business people, but to some extent this was also true for a later group of historians who attacked the racist assumptions of the Dunning school. The books and articles of Carter G. Woodson, Abram Harris, Merah Stuart, Luther Porter Jackson, John Hope Franklin, Vernon Lane Wharton, and other revisionist authors included only brief notations of blacks

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¹ Frederick Law Olmsted, *Journey in the Seaboard Slave States, in the Years 1853–1854, With Remarks on their Economy* (New York, 1856), 632–33. Creoles of color were persons of French or Spanish and Negroid descent born in the Americas. In Louisiana, the term "Creole" was also applied to whites culturally related to the original French settlers. See Ira Berlin, "Time, Space, and the Evolution of Afro-American Society on British Mainland North America," *AHR*, 85 (February 1980): 45.

² Calvin Dill Wilson, "Black Masters: A Side-Light on Slavery," *North American Review*, 181 (November 1905): 685–98; John Russell, "Colored Freemen as Slave Owners in Virginia," *Journal of Negro History*, 1 (June 1916): 233–42. See also Wilson, "Negroes Who Owned Slaves," *Popular Science Monthly*, 81 (November 1912): 483–94.

who had acquired substantial amounts of property.³ Even with the explosion of research on various aspects of the black experience during the late 1960s and 1970s, historians seemed more interested in racial exploitation, black culture and black consciousness, and the political activities of blacks during Reconstruction than with those who achieved financial success.⁴

The appearance of David Rankin's article "The Impact of the Civil War on the Free Colored Community of New Orleans" and Gary B. Mills's book *The Forgotten People: Cane River's Creoles of Color* in 1977, however, ignited a new interest in this subject. Subsequent studies also asked how blacks, in the midst of slavery, racism, and white oppression, acquired substantial amounts of wealth. In his study of a Plaquemines Parish sugar planter, Andrew Durnford, David Whitten uncovered rare documents about one of the wealthiest free black planters in the slaveholding states. In a biography of William Ellison, a South Carolina cotton gin maker, Michael Johnson and James Roark unraveled the remarkable career of a former slave who became the South's only large-scale, pre-Civil War black manufacturer. In a detailed examination of free black slave masters in South Carolina, Larry Koger pointed to the entrepreneurial spirit among free blacks who held their fellows in bondage. And, in his probing analysis of urban race relations, Howard N.

³ Walter L. Fleming, *Civil War and Reconstruction in Alabama* (New York, 1905); C. Mildred Thompson, *Reconstruction in Georgia: Economic, Social, Political, 1865-1872* (New York, 1915); J. G. De Roulhac Hamilton, *Reconstruction in North Carolina* (New York, 1914); James W. Garner, *Reconstruction in Mississippi* (New York, 1901); William A. Dunning, *Reconstruction, Political and Economic, 1865-1877* (New York, 1907); Carter G. Woodson, *Free Negro Owners of Slaves in the United States in 1830; Together with Absentee Ownership of Slaves in the United States in 1830* (Washington, D.C., 1924); Woodson and Arnett G. Lindsay, eds., *The Negro as a Businessman* (Washington, D.C., 1929); Abram L. Harris, *The Negro as Capitalist: A Study of Banking and Business among American Negroes* (Philadelphia, 1936); Merah S. Stuart, *An Economic Detour: A History of Insurance in the Lives of American Negroes* (New York, 1940); Luther Porter Jackson, *Free Negro Labor and Property Holding in Virginia, 1830-1860* (New York, 1942); and "The Virginia Free Negro Farmer and Property Owner, 1830-1860," *Journal of Negro History*, 24 (October 1939): 390-489; John Hope Franklin, *The Free Negro in North Carolina, 1790-1860* (Chapel Hill, N.C., 1943); and "The Free Negro in the Economic Life of Ante-Bellum North Carolina," *North Carolina Historical Review*, 19 (July and October 1942), 239-59, 359-75; Vernon Lane Wharton, *The Negro in Mississippi, 1865-1890* (Chapel Hill, N.C., 1947).

⁴ Lawrence W. Levine, *Black Culture and Black Consciousness: Afro-American Folk Thought from Slavery to Freedom* (New York, 1977); Herbert Gutman, *The Black Family in Slavery and Freedom, 1750-1925* (New York, 1976); George P. Rawick, *From Sundown to Sunup: The Making of the Black Community* (Westport, Conn., 1972); John W. Blassingame, *The Slave Community: Plantation Life in the Antebellum South* (New York, 1971); Eugene Genovese, *Roll, Jordan, Roll: The World the Slaves Made* (New York, 1974); Arnold Taylor, *Travail and Triumph: Black Life and Culture in the South since the Civil War* (Westport, Conn., 1976); Charles Vincent, *Black Legislators in Louisiana during Reconstruction* (Baton Rouge, La., 1976); Thomas Holt, *Black over White: Negro Political Leadership in South Carolina during Reconstruction* (Urbana, Ill., 1977); David Rankin, "The Origins of Black Leadership in New Orleans during Reconstruction," *Journal of Southern History*, 40 (August 1974): 417-40. This has also been true for recent economic historians who have paid only slight attention to property ownership and have said virtually nothing about prosperous blacks. See Robert Higgs, *Competition and Coercion: Blacks in the American Economy, 1865-1914* (London, 1977); Stephen J. DeCanio, *Agriculture in the Postbellum South: The Economics of Production and Supply* (Cambridge, Mass., 1974); Ralph Shlomowitz, "Planter Combinations and Black Labour in the American South, 1865-1880," *Slavery and Abolition: A Journal of Comparative Studies*, 9 (May 1988): 72-84; Jonathan Wiener, *Social Origins of the New South: Alabama, 1860-1885* (Baton Rouge, La., 1978); Jay R. Mandle, *The Roots of Black Poverty: The Southern Plantation Economy after the Civil War* (Durham, N.C., 1978); Gerald David Jaynes, *Branches without Roots: Genesis of the Black Working Class in the American South, 1862-1882* (New York, 1986); Roger L. Ransom and Richard Sutch, *One Kind of Freedom: The Economic Consequences of Emancipation* (London, 1977); Harold Woodman, "Sequel to Slavery: The New History Views the Postbellum South," *Journal of Southern History*, 43 (November 1977): 523-54. Although characteristic of a slightly later time, one exception to these trends was Walter Wear, *Black Business in the New South: A Social History of the North Carolina Mutual Life Insurance Company* (Urbana, Ill., 1973).

Rabinowitz noted the emergence of a small black business elite in five cities of the postwar South.⁵

Despite these recent studies, we still have only a vague understanding of blacks who reached the upper economic levels. We know comparatively little about how much land and other property they accumulated, how their wealth changed from one generation to the next, and how their holdings compared with those of other blacks and whites. We know even less about the group's socio-demographic profile—mulatto and black, male and female, rural and urban—and how it changed over time in various sections of the South. In addition, important questions remain unanswered. What effect did repressive laws and institutions have on prosperous blacks? What occupations did they follow? What were their social and cultural attitudes? Their relationship with whites? How did their position change as a result of the Civil War? In short, how and why did a small group of blacks rise to the top of the economic hierarchy, and what does their doing so tell us about antebellum black culture and its post-bellum consequences?

In describing this group, contemporaries and later historians have used such terms as “colored aristocracy,” “mulatto elite,” “free Negro elite,” “upper caste” free Negroes, and “upper-class” blacks. Such phrases often create more problems than they solve.⁶ While “prosperity” depends on time, location, and circumstance, perhaps the best way to analyze this group is by formulating a “wealth model” based on property ownership. This, too, presents special problems, since the value of land and other property fluctuated across the South during different periods, and the U.S. population censuses do not offer region-wide property estimates until 1850 through 1870. Generally, however, someone with at least \$2,000 worth of real estate was considered relatively prosperous during the entire hundred years under discussion. This essay therefore focuses primarily on those who reached this level of property ownership. For the period before 1850, only scattered information can be obtained on the group worth \$2,000 and more extrapolated from probate court records, estate inventories, tax assessment lists, and inferred from statistics on black

⁵ David Rankin, “The Impact of the Civil War on the Free Colored Community of New Orleans,” *Perspectives in American History*, 11 (1977–78): 379–418; Gary B. Mills, *The Forgotten People: Cane River's Creoles of Color* (Baton Rouge, La., 1977); David O. Whitten, *Andrew Durnford: A Black Sugar Planter in Antebellum Louisiana* (Natchitoches, La., 1981); Michael P. Johnson and James L. Roark, *Black Masters: A Free Family of Color in the Old South* (New York, 1984); and eds., *No Chariot Let Down: Charleston's Free People of Color on the Eve of the Civil War* (Chapel Hill, N.C., 1984); Larry Koger, *Black Slaveowners: Free Black Slave Masters in South Carolina, 1790–1860* (Jefferson, N.C., 1985); Howard N. Rabinowitz, *Race Relations in the Urban South, 1865–1890* (New York, 1978). See also Whittington B. Johnson, “Free Blacks in Antebellum Savannah: An Economic Profile,” *Georgia Historical Quarterly*, 64 (Winter 1980): 418–31; Philip J. Schwarz, “Emancipators, Protectors, and Anomalies: Free Black Slaveowners in Virginia,” *Virginia Magazine of History and Biography*, 95 (July 1987): 317–38; Juliet E. K. Walker, “Racism, Slavery, and Free Enterprise: Black Entrepreneurship in the United States before the Civil War,” *Business History Review*, 60 (Autumn 1986): 343–82; Willard B. Gatewood, Jr., “Aristocrats of Color: South and North: The Black Elite, 1880–1920,” *Journal of Southern History*, 54 (February 1988): 3–20; Janet S. Hermann, *The Pursuit of a Dream* (New York, 1981); George C. Wright, *Life behind A Veil: Blacks in Louisville, Kentucky, 1865–1930* (Baton Rouge, 1985); Robert Engs, *Freedom's First Generation: Black Hampton, Virginia, 1861–1890* (Philadelphia, 1979); Loren Schweninger, ed., *From Tennessee Slave to St. Louis Entrepreneur: The Autobiography of James Thomas* (Columbia, Mo., 1984).

⁶ Cyprian Clamorgan, *The Colored Aristocracy of St. Louis* (St. Louis, Mo., 1858), 16, 19; Berlin, *Slaves without Masters*, 298; Koger, *Black Slaveowners*, 170; Gatewood, “Aristocrats of Color,” 4–5; Johnson and Roark, *Black Masters*, 142, 189, 208–18, 409; Rabinowitz, *Race Relations in the Urban South*, 240–43, 248–52, 402; E. Horace Fitchett, “The Traditions of the Free Negro in Charleston, South Carolina,” *Journal of Negro History*, 25 (April 1940), reprinted in John H. Bracey, Jr., August Meier, and Elliott Rudwick, eds., *Free Blacks in America, 1800–1860* (Belmont, Calif., 1971), 29; Robert L. Harris, Jr., “Charleston's Free Afro-American Elite: The Brown Fellowship Society and the Humane Brotherhood,” *South Carolina Historical Magazine*, 82 (October 1981): 289–310.

slaveownership. Much more detail is available for the middle years. In both cases, a "wealth model" allows us to examine the black economic elite with a degree of precision not previously possible.⁷

The broad temporal and spatial dimensions of the pages that follow require a few comments about structure and organization. While any chronological demarcation is partially arbitrary, the article captures two important transitions—from the late 1830s to the early 1840s, and from the 1860s to the early 1870s. Similarly, while geographical groupings tend to minimize intraregional diversity, the dissimilarity between prosperous blacks in the Upper and the Lower South—in origins, average wealth, occupations, phenotype (black or mulatto), gender, and residency (rural versus urban)—lends itself to comparative analysis and forms a sub-regional context.⁸ Finally, to understand fully the changes occurring from one generation to the next—the rise, leveling, and relative decline in the lower states; the slow, early growth and rapid middle-period expansion in the upper states—it is necessary to examine the different social and cultural values among prosperous blacks in both parts of the South as well as their changing relationship with whites.

THE ORIGIN OF THE MOST PROSPEROUS group of blacks can be traced to the late eighteenth and early nineteenth centuries, when some white men in the Lower South took black women as their sexual partners and bequeathed them, or their mulatto children, land and slaves. Along the Atlantic seaboard, where British traditions and racial prejudices prevailed, only a few dozen black families acquired landed estates in such a manner, but along the Gulf Coast, and in Louisiana, where Spanish and French customs (as well as the small number of white women) encouraged interracial unions, several hundred free black families received or inherited property from whites. While marriage between whites and slaves, or free blacks, was illegal in every state, mixed racial couples became so common in

⁷ In general, when the true value as opposed to the assessed value (usually, one-third to one-half of the actual value) of an individual's land can be determined, the lower limit of \$2,000 has been used throughout this essay. The comparative value of \$2,000 worth of land varied during different time periods, in the eastern and western states, during periods of inflation and deflation. It was worth comparatively more in the 1830s than the 1850s, and comparatively less in the 1860s than the 1850s. I made several attempts to create an adjusting-scale wealth model, analyzing those with one amount of real estate in one period, then adjusting that amount to what it would be worth during another period, or in a different section of the South. In 1870, for instance, \$2,034 worth of land was worth \$1,440 when adjusted to 1860 prices. But each time I tried such a scaled model, it foundered on the number of adjustments that would need to be made, the crudeness of nineteenth-century wealth statistics, and the paucity of comparative data for various sections of the South during various periods. I chose \$2,000 as a lower limit because it would be accurate to describe someone with at least that amount as entering a prosperous group during the period under study. In 1850, in the nation as a whole, where we do have relatively accurate comparative data for males aged twenty and over, those with \$2,000 in realty had reached the upper 13 percent of the population; in 1870, they were among the top 20 percent. There were blacks who controlled relatively large amounts of personal property (including a few slaves who owned property) and no real estate, but their numbers are few. When appropriate, I have examined personal and total estate (real and personal) holdings among prosperous blacks, especially with regard to slaveownership (listed as personal property except in Virginia and some Louisiana parishes). See Lee Soltow, *Men and Wealth in the United States, 1850–1870* (New Haven, Conn., 1975), 65, 186.

⁸ In this essay, the Lower South includes South Carolina, Georgia, Florida, Alabama, Mississippi, Arkansas, Louisiana, and Texas; the Upper South includes Delaware, Maryland, the District of Columbia, Virginia, North Carolina, Kentucky, Tennessee, and Missouri. References to the Deep South and the Border South generally refer to the same demarcation. See Berlin, "Time, Space, and the Evolution of Afro-American Society," 45; and "The Structure of the Free Negro Caste in the Antebellum United States," *Journal of Social History*, 9 (Spring 1976): 297–319; Soltow, *Men and Wealth*, 186.

Louisiana that an institution called *placage*—white men contracting to live with black women and provide them with financial support—became firmly established. In addition, during the early 1800s, white (and free colored) immigrants from the Caribbean, often of French background, arrived in the United States with black spouses or, upon arrival, took free women of color or slaves as their partners.⁹

Whatever the specific circumstances, and despite the range of traditions across the Lower South, the vast majority of free people of color who reached the upper economic levels during these early years was of mixed racial ancestry. Most were children or grandchildren of white planters or merchants and slave women. They had received land, slaves, and other property from their white relatives. In South Carolina, the small group of prosperous artisans in Charleston during the 1790s, as well as those who in later years established successful businesses or became rice planters, were virtually all directly related to whites. The wealthiest free black in the state, James Pendarvis, who owned 3,250 acres of land, was the son of a white planter and a slave woman. Other prosperous free people of color, including John Holman, Jr., Elias Collins, Jehu Jones, William Penceel, William Ellison, and Margaret Noisette, were descendants of white men and slave or free black women. In Georgia, Anthony Odingsells, one of the largest black property owners, received his land and nine slaves from Charles Odingsells, an officer in the American Revolution. The most prominent “colored Creole family” in Florida, the Pons family, claimed descent from two Spanish officers. In Alabama, the two largest black landholders, Zeno and Basile Chastang, were the children of Dr. John Chastang, a prominent surgeon who had served as a medical consultant at the Spanish fort of San Esteban de Tombeche. Similarly, in Mississippi and Louisiana, prosperous free people of color were of mixed racial heritage.¹⁰

⁹ E. Horace Fitchett, “The Origin and Growth of the Free Negro Population of Charleston, South Carolina,” *Journal of Negro History*, 26 (October 1941): 425–26; Laura Foner, “The Free People of Color in Louisiana and St. Domingue: A Comparative Portrait of Two Three-Caste Slave Societies,” *Journal of Social History*, 3 (Summer 1970): 408–11; James Robertson, ed., *Louisiana under the Rule of Spain, France, and the United States, 1785–1807: Social, Economic, and Political Conditions of the Territory represented in the Louisiana Purchase*, 2 vols. (Cleveland, Ohio, 1910–11), rpt. edn., Freeport, N.Y., 1969), 1: 218–19; Winthrop Jordan, *White over Black: American Attitudes toward the Negro, 1550–1812* (Chapel Hill, N.C., 1968), 77–81.

¹⁰ Fitchett, “Traditions of the Free Negro in Charleston, South Carolina,” 141; Harris, “Charleston’s Free Afro-American Elite,” 308; Koger, *Black Slaveowners*, 10–14, 23, 135, 165; Legislative Records, Petition of [John Holman] to the South Carolina General Assembly, October 3, 1791, South Carolina Department of Archives and History, Columbia, S.C. [hereafter, SCDAH]; *ibid.*, Memorial of Thomas Cole, Peter Bassnett Mathews, and Mathew Webb to the South Carolina General Assembly, January 13, 1791, no. 181, SCDAH; *ibid.*, Petition of John L. Wilson [Jehu Jones’s guardian] to the South Carolina General Assembly, December 6, 1823, SCDAH; Records of the County Probate Court [hereafter, RCPC], Charleston Co., S.C., Miscellaneous Land Records, pt. 87, bks. R6–S6 (February 16, 1796), 161–62, in Museum of Early Southern Decorative Arts, Winston-Salem, N.C. [hereafter, MESDA]; *ibid.*, pt. 88, bks. T6–U6 (October 11–12, 1794), 520–22, in MESDA; Brent H. Holcomb, ed., “1786 Tax Returns,” *South Carolina Magazine of Ancestral Research*, 9 (Spring 1981): 73; Philip D. Morgan, “Black Life in Eighteenth-Century Charleston,” in *Perspectives in American History*, n.s., 1 (1984): 191, 193, 205, 216, 222; Extract of the Will of Philip Stanislas Noisette, ca. 1830, in Noisette Family Papers, South Caroliniana Library, Columbia, S.C.; RCPC, Effingham Co., Ga., Deeds, bk. G (January 1, 1806), 445–46, in Charles Odingsells Papers, Georgia Historical Society, Savannah, Ga.; David Thomas, “The Free Negro in Florida before 1865,” *South Atlantic Review*, 10 (October 1911): 336; Jack D. L. Homes, “The Roles of Blacks in Spanish Alabama: The Mobile District, 1780–1813,” *Alabama Historical Quarterly*, 37 (Spring 1975): 10–11; Legislative Records, Petition of L. Rowan, John Smith, *et al.* [concerning the Barland family], to the Mississippi General Assembly, ca. 1830, Record Group 47, box 19, Mississippi Department of Archives and History, Jackson, Miss.; *General Index of All Successions, Opened in the Parish of Orleans, From the Year 1805, to the Year 1846*, comp. P. M. Bertin (New Orleans, 1849), *passim*; Records of the Parish Probate Court [hereafter, RPPC], Pointe Coupée Parish, La., Successions, no. 176, April 5, 1839; Deed of Emancipation for Jean Meullion, February 21, 1776, in Meullion Family Papers,

Even with the advantage of inheritance, it took energy, industry, and business acumen for these people to maintain their property holdings. In towns and cities, free men and women of color took advantage of the continued demand for service businesses, the relatively small numbers of skilled whites and immigrants, and the general appreciation in city property values to expand their estates. In rural areas, they took advantage of the economic expansion in the West, rising prices for slaves, and increasing real estate values. Some speculated in city property or expanded their farm acreage, while others simply watched as the value of the land they had acquired years before appreciated dramatically in value. Improved farm acreage in Louisiana, especially along a river or bayou, went from \$2 an acre at the time of statehood in 1812 to \$10, \$25, \$50 or more during the 1820s and 1830s. They also witnessed a rise in the value of their personal holdings, including wagons, livestock, tools, machinery, and slaves. Those who astutely managed their businesses or expanded their farm acreage were able to increase their wealth substantially.¹¹

To sustain their economic activities, free people of color acquired increasing numbers of slaves. Urban artisans—carpenters, bricklayers, stonemasons, mechanics—purchased black apprentices, hod carriers, and helpers; merchants and business people bought haulers, carters, and stock boys; plantation owners purchased house servants, cooks, mechanics, and field hands. By 1830, approximately 1,556 free black masters in the Deep South owned a total of 7,188 slaves. Representing about 42 percent of the black owners in the South, they owned 60 percent of the black-owned slaves. In the Charleston District, 407 owners held a total of 2,195 slaves. In New Orleans, there were 753 free black owners, including 25 who owned at least 10 bondsmen and women and another 116 who owned between 5 and 9 slaves. Although some of these slaveholders owned members of their own families, or loved ones, unable to free them by law, in 8 rural Louisiana sugar and cotton parishes, 43 Creoles of color (1.2 percent of the black slaveholders in the South) owned a total of 1,327 blacks, or 1 out of 9 slaves owned by blacks. In St. John the Baptist Parish, 3 plantation owners held 139 blacks in bondage—an average of 46 slaves each; in Pointe Coupée Parish, 8 planters held 297 slaves, an average of 37 slaves each. In 1830, approximately 1 out of 4 free black families in the region was a slaveholder.¹²

In their attitudes toward their bondsmen and treatment of their slaves, these

Louisiana State University, Baton Rouge, La.; RPPC, Natchitoches Parish, La., Successions, no. 375, July 26, 1839.

¹¹ Thomas S. Berry, *Western Prices before 1861* (Cambridge, Mass., 1943), 186; Last Will and Testament of Richard Holloway, October 19, 1842, in Holloway Family Papers, College of Charleston, Charleston, S.C.; Helen Catterall, ed., *Judicial Cases Concerning American Slavery and the Negro*, 5 vols. (Washington, D.C., 1932), 3: 292, 589, 611–12; RPPC, St. Landry Parish, La., Conveyances, bk. H (March 22, 1830), 32–33; Herbert Sterkx, *The Free Negro in Ante-bellum Louisiana* (Rutherford, N.J., 1972), 204–07.

¹² Koger, *Black Slaveowners*, 11–12, 20–21, 218–23; Johnson, “Free Blacks in Antebellum Savannah,” 423–24; Receipt, William Shipp to Jean Meullion, February 5, 1811, Meullion Family Papers; Mills, *Forgotten People*, 57; RPPC, Natchitoches Parish, La., Successions, no. 375, July 26, 1839; Woodson, *Free Negro Owners of Slaves in the United States in 1830*, *passim*; R. Halliburton, Jr., “Free Black Owners of Slaves: A Reappraisal of the Woodson Thesis,” *South Carolina Historical Magazine*, 76 (July 1976): 129–35. Because not all free blacks who had slaves listed in the same household were owners of those slaves, several of Woodson’s listings are incorrect. The forty-three Creoles of color in Louisiana cited above have been checked in parish records. I have used Koger’s findings for South Carolina and the compilations of Halliburton, excluding the Arkansas Territory, for the other states in the Lower South. The debate concerning the extent of “benevolent” versus “commercial” ownership has generally focused on the entire South and thus minimized the diversity among black slaveowners in different regions during different time periods.

slaveowners differed little from their white neighbors. While occasionally freeing a slave as a reward for long years of service or purchasing blacks for personal reasons, free mulatto owners generally bought and sold slaves as a matter of economic necessity. Plaquemines Parish sugar planter Durnford, the mulatto son of English-born Thomas Durnford, an early settler in Louisiana, journeyed all the way to Richmond, Virginia, in 1835 to acquire a group of blacks. "I have two or three bargains on hand, butt so high, that I dare nott come to a conclusion," he lamented, "women of 32 her daughter of 12, a boy of 7, a boy of 3 for [\$]1350." In Charleston, Savannah, Pensacola, Mobile, New Orleans, and in scattered rural counties and parishes, property-owning free people of color bought, sold, mortgaged, willed, traded, and transferred fellow blacks, demanded long hours in the workshops and fields, and severely disciplined recalcitrance. A few seemed as callous as the most profit-minded whites. South Carolina planters John and William Holman, African-born mulatto sons of English slave trader John Holman, established a "factory" on the Rio Pong River, north of Sierra Leone, and for nearly a quarter-century, reaped huge profits buying and selling fellow Africans.¹³

To protect their property, free people of color in the Lower South formed small, tightly knit social and cultural clans, linking their families through intermarriage. In South Carolina, the Holman and Collins families were related by ties of kinship and marriage, as were the Ellison, Weston, Holloway, Johnson, and Bonneau families. In Charleston, the same was true for the Cole-Seymour, Garden-Mitchell, Inglis-Glover, Lee-Seymour, and McKinlay-Huger families. The free black of "status," one later observer noted, chose a marital partner according to three criteria: economic position, "cultural status," and free, mixed-blood ancestry. In Mississippi, John Barland, a wealthy planter, married Mary Fitzgerald, the daughter of a prosperous free mulatto in Natchez. Among prosperous Creoles of color in Louisiana, endogamous marriages were almost universal. Antoine Decuir and Antoine Dubuclet, the richest blacks in Pointe Coupée Parish, signed formal contracts concerning their children. In the case of Decuir's son, Antoine, Jr., and Dubuclet's daughter, Josephine, they drew up a four-page document (in French) specifying the size of the dowry and arrangements for the distribution of property. Similar contracts, or verbal agreements, were made between the Donatto, Meullion, Simien [Simon], Guillory, and Lemmelle families in St. Landry Parish; the Conant, Metoyer, Rogues, and Llorens in Natchitoches; the Reggio, Oliver, and Leonard families in Plaquemines; and the Bienville, Ricard, and Turpin families in East Baton Rouge. One local court judgment described the Decuir, Deslondes, Honoré, and Dubucelet families in West Baton Rouge and Pointe Coupée parishes as being "all free persons of color, Relations & friends."¹⁴

¹³ RPPC, Iberville Parish, La., Conveyances, bk. G (April 26, 1819), 19–20; Andrew Durnford to John McDonough, June 25, July 5, 1835, in David O. Whitten, ed., "Slave Buying in Virginia as Revealed by Letters of a Louisiana Negro Sugar Planter," *Louisiana History*, 11 (Summer 1970): 239–40; Legislative Records, Petition of [John Holman] to the South Carolina General Assembly, October 3, 1791, SCDAH; Koger, *Black Slaveowners*, 110, 119, 254; Tinsley L. Spraggins, "The History of the Negro in Business prior to 1860," (M.A. thesis, Howard University, 1935), 33.

¹⁴ Marriage Book, St. Phillip's Church, Charleston, S.C., in Fitchett, "Origin and Growth," 431–32; RCPC, Charleston, S.C., Estates, no. 229–6, December 30, 1874; William Hogan and Edwin Davis, eds., *William Johnson's Natchez: The Ante-bellum Diary of a Free Negro* (Baton Rouge, La., 1951), 11, 43, 334, 399; Legislative Records, Petition of Andrew Barland to the Senate and House of Representatives of Mississippi, ca. 1824, Record Group 47, boxes 16–17, Mississippi Department of Archives and History; "A Contract of Marriage between JOSEPH METOYER and MARIE LODOISKA LLORENS," January 28, 1840, Cane River Collection, Historic New Orleans Collection, New Orleans, La.; RPPC, Pointe Coupée Parish, La., Marriage Contract, February 26, 1835; U.S. Manuscript Population Census [hereafter, USMSPC], St. Landry Parish, La., 1860, *passim*. In 1860, census takers in St. Landry Parish listed the

Benefiting from their exclusive family and friendship networks, their close associations with whites, the French and Spanish traditions along the Gulf Coast, and the westward growth of the region's economy, prosperous mulatto families in the region greatly expanded their property holdings during the early decades of the nineteenth century. While precise statistics are fragmentary, a tax assessment list for New Orleans symbolized the extent of the growth: a small number of property owners early in the century had increased by 1836 to 855 free persons of color (approximately one-third of the free black family heads), who paid taxes on property worth a total of \$2,462,470, nearly \$3,000 per property owner.¹⁵

During the 1840s and 1850s, this early expansion slowed considerably, as prosperous free people of color experienced the effects of a prolonged depression (1837–1843), increased competition from native and foreign-born white artisans (who argued that blacks should not be allowed into skilled occupations because they undercut whites), and, in rural areas, from the perennial problems of drought, flooding, fluctuating prices, and currency depreciation. A series of new state and local laws was designed to curtail their activities. Coinciding with these economic and political problems, a number of free people of color who had acquired their holdings during the early years reached their sixties and seventies. As they died, their estates were broken up and distributed among heirs or sold to satisfy creditors.¹⁶

Despite such difficulties, the late antebellum era was not a period of decline for the region's most prosperous blacks. Although some areas, including Charleston, New Orleans, and a few Louisiana parishes, saw a drop in the number of black slaveowners and in the number of slaves they owned, in other areas, including Mobile, and several Louisiana cotton and sugar parishes, the opposite was true. Similarly, while the number of free blacks who controlled at least \$2,000 in real estate declined in the Orleans, Jefferson, Natchitoches, and St. John the Baptist parishes during the 1850s, in other parishes and outside of Louisiana, increasing numbers of free blacks were able to take advantage of the general prosperity during the decade and to enter the most prosperous group for the first time.¹⁷ In

surnames of spouses in most households. RPPC, St. Landry Parish, La., Marriage Certificate, March 25, 1796, Meullion Family Papers; *ibid.*, Natchitoches Parish, La., Successions, no. 344, September 7, 1838; *ibid.*, Plaquemines Parish, La., Successions, no. 167, May 12, 1840; *ibid.*, East Baton Rouge Parish, La., Successions, no. 640, August 14, 1855; *ibid.*, West Baton Rouge Parish, La., Successions, no. 176, July 18, 1829.

¹⁵ Robert Reinders, "The Free Negro in the New Orleans Economy, 1850–60," *Louisiana History*, 6 (Summer 1965): 280.

¹⁶ Richard Tansey, "Out-of-State Free Blacks in Late Antebellum New Orleans," *Louisiana History*, 22 (Fall 1981): 378; Koger, *Black Slaveowners*, 128–29, 226–27; USMSPC, Charleston, S.C., 1850, 320. In this and subsequent references to the USMSPC, I have used the printed or hand-written page numbers in the upper right-hand corner of the right-hand page. The unnumbered pages facing the numbered pages are cited as the same page. *Ibid.*, Chatham Co., Ga., Savannah, 1850, 324; RCPC, Chatham Co., Ga., Estates, no. M-395, April 6, 1857; Johnson, "Free Blacks in Antebellum Savannah," 424; USMSPC, Adams Co., Miss., 1860, 120; RPPC, Natchitoches Parish, La., Estates, no. 355, September 7, 1838; Estates, no. 692, July 25, 1851; Estates, no. 606, October 14, 1847; USMSPC, St. Landry Parish, La., 1850, 15, 17; Ira Berlin and Herbert G. Gutman, "Natives and Immigrants, Free Men and Slaves: Urban Workingmen in the Antebellum American South," *AHR*, 88 (December 1983): 1198.

¹⁷ Computed from USMSPC, 1850, 1860; Koger, *Black Slaveowners*, 20–21; Mills, *Forgotten People*, 218; USMSPC, New Orleans, La., 1st Mun., 7th Ward, 1850, 376, 396; Robert Reinders, "The Decline of the New Orleans Free Negro in the Decade before the Civil War," *Journal of Mississippi History*, 24 (April 1962): 95–96. In Charleston District, including the city and surrounding rural parishes, the number of free blacks who possessed slaves declined from 402 in 1840 to 137 in 1860, and the number of slaves they owned fell from 2001 to 544, or 73 percent. In other South Carolina districts, and in several Louisiana parishes, black slaveholders sold off some of their slave property. In the Crescent City, the

TABLE 1
Real Estate Holdings among Prosperous Free People of Color in the
Lower South, 1850 and 1860 (those with at least \$2,000)

State	Owners		Average Real Estate Holdings	
	1850	1860	1850	1860
Alabama	12	32	\$3,817	\$ 3,691
Arkansas	4	**	2,500	**
Florida	2	8	4,000	3,825
Georgia	5	13	5,200	3,585
Louisiana	504	472	7,922	10,311
Mississippi	6	13	4,033	4,685
South Carolina	47*	162	4,411	4,723
Texas	3	6	5,667	5,133
Total	583	706	\$7,428	\$ 8,384

* = probable census undercount

** = none

SOURCE: Computed from the U.S. Manuscript Population Census, 1850, 1860.

Georgia, Florida, Alabama, Mississippi, and Texas, while the free black population rose less than 10 percent between 1850 and 1860, the number of realty holders with \$2,000 or more rose from 28 to 72, or 157 percent, and the value of their real estate rose from \$121,000 to \$287,000. Even in areas where free blacks seemed to experience the greatest difficulties, some energetic and enterprising planters and business people substantially improved their economic positions. Between 1850 and 1857, New Orleans mortician Pierre Casenave, for example, quadrupled the size of his business operations.¹⁸ As Table 1 reveals, despite general economic difficulties and political turbulence, prosperous blacks in the lower states maintained a stable economic position during the tumultuous decade before the Civil War.

Although comparable data for whites are not available, some tentative conclusions can be advanced. First, the slaveholding regions of the lower Mississippi river valley where the wealthiest whites in the South resided were also where the most

number of free persons of color who owned at least \$2,000 in realty went from 311 in 1850 to 263 a decade later, and the value of their holdings, despite rising real estate prices, remained approximately the same (\$2,188,000 vs. \$2,317,300). The rise in city property values greatly exceeded the 6 percent valuation increase and did little to offset the 15 percent decline in the number of free people of color who had reached the upper wealth levels.

¹⁸ USMSPC, Charleston, S.C., 1850, 91, 98, 110, 118, 126–28, 186, 223, 288, 320, 347, 356; 3d Ward, 1860, 283; 4th Ward, 1860, 307–12, 327, 332, 334, 404; 5th Ward, 1860, 376, 385; 6th Ward, 1860, 432; 7th Ward, 1860, 466, 468, 475; 8th Ward, 1860, 493, 510, 523; *List of Tax Payers of the City of Charleston for 1860* (Charleston, S.C., 1861), 315–34; RCPC, Charleston, S.C., Estates, no. 289–25, June 15, 1876; Records of the Comptroller General, Free Negro Tax Book, Charleston District, S.C., 1843, SCD4H; USMSPC, Sumter Co., S.C., 1850, 285; 1860, 133. In 1852, Ellison paid \$9,560 for Keith Hill and Hickory Hill, plantations totaling 540 acres, bringing his total land holdings to more than 1,000 acres. I have therefore estimated Ellison's realty at \$15,000, rather than the \$8,300 listed in the 1860 census. Koger, *Black Slaveholders*, 37–38, 62, 121–23, 132, 136, 144–45; Johnson and Roark, *Black Masters*, 70, 124–27; USMSPC, Emanuel Co., Ga., 1860, 960; *ibid.*, Hamilton Co., Fla., 1860, 597; *ibid.*, Madison Co., Ala., Huntsville, 188; *ibid.*, Adams Co., Miss., Natchez, 1850, 14; 1860, 44, 120; *ibid.*, Jefferson Co., Tex., 1850, 481, 497, 499; Andrew Muir, "The Free Negro in Jefferson and Orange Counties, Texas," *Journal of Negro History*, 35 (April 1950): 186, 191, 206; Walker, "Racism, Slavery, and Free Enterprise," 354, 361–62; U.S. Manuscript Agricultural Census, Iberville Parish, La., 1850, 81; USMSPC, Iberville Parish, La., 1860, 261, 263–64, 267, 280.

prosperous free people of color owned their farms, plantations, and business enterprises. The average estate (real and personal property) among adult white freemen in Pointe Coupée Parish in 1860 was worth \$14,290, in West Baton Rouge \$18,830, in Iberville \$18,330, in Jefferson \$4,470, in Plaquemines \$8,570, in St. John the Baptist \$8,340, and in Orleans \$2,170. The average estate among free black heads of family in these same parishes included Pointe Coupée at \$5,528, West Baton Rouge at \$3,848, Iberville at \$17,503, Jefferson at \$2,270, Plaquemines at \$3,686, St. John the Baptist at \$3,568, and Orleans at \$1,567. It was the substantial wealth of affluent slaveowning whites and prosperous free people of color in these parishes that pushed these averages to such high levels. Indeed, it is remarkable that in New Orleans free blacks achieved an economic standing not much below whites in the city. Second, the proportion of free people of color in the lower states who owned at least some property—54 percent—was not too far below the 66 percent for whites in the South. And, third, the figure of \$1,500 mean estate among free black heads of household in the Lower South in 1860 was nearly two-fifths of the mean of \$4,000 among adult white male Southerners.¹⁹

Prosperous free people of color were able to maintain their high economic standing in large measure because they did not pose a threat to the South's "peculiar institution." Edwin C. Holland, a leading South Carolina editor, noted this as early as 1822, when he wrote that free mulattoes were industrious, sober, and hardworking artisans and farmers who had large families and considerable amounts of property: "so far as we are acquainted with their temper and disposition of their feelings [they] abhor the idea of association with blacks in any enterprise that may have for its object the revolution of their condition." In addition, free blacks who owned barber shops, butcher shops, mercantile establishments, and tailoring businesses or who worked as carpenters, coopers, builders, masons, cigarmakers, and bootmakers provided valuable services to whites in their communities. "Their labor is indispensable to us in this neighbourhood," noted James Rose, William J. Grayson, Benjamin Huger, and a group of other prominent whites in South Carolina in 1860. Not only did free blacks "command the respect of all respectable men" but they were also "good citizens" who demonstrated "patterns of industry, sobriety, and irreproachable conduct"; in Charleston alone, they owned property worth \$500,000. Thus, separating themselves from slaves and less fortunate free blacks, forming exclusive family and friendship networks, and aligning themselves with the planter aristocracy, affluent free people of color in the lower states pinned their hopes and their future prosperity on the very system that held other blacks in slavery.²⁰

¹⁹ Soltow, *Men and Wealth*, 44, 166–68. Unfortunately, comparative data are available only for adult white males. To compare white male wealth holders with free black male wealth holders would exclude 30 percent of the black estate owners who were women. Consequently, for a rough comparison, I have used free black heads of households. Estimates of free black family heads are derived from the general population; I have estimated approximately one out of five in this group. Thus Pointe Coupée Parish, with a total free black population of 721, had approximately 144 families; West Baton Rouge, with a total free black population of 113, had 23 families; Iberville, with a population of 188, had 38 families; Jefferson, with a population of 287, had 57 families; Plaquemines, with a population of 514, had 103 families; St. John the Baptist, with a population of 299, had 60 families; and Orleans, with a population of 10,939, had approximately 2,188 free black families. In the Lower South, there were approximately 7,391 free black families. *Population of the United States in 1860; Compiled from the Original Returns of the Eighth Census* (Washington, D.C., 1864), 189, 191; Berlin, *Slaves without Masters*, 136. The total estates were computed from USMSPC, 1860.

²⁰ Legislative Records, Petition of James Rose, William Grayson, Benjamin Huger, *et al.*, to the South Carolina Senator, ca. 1860, SCDAH; Edward Holland, *A Refutation of the Calumnies Circulated against the Southern and Western States, Respecting the Institution and Existence of Slavery among Them* (Charleston, S.C.,

IN THE UPPER SOUTH, prior to 1840, only a tiny number of free blacks—fewer than one hundred families—acquired more than \$2,000 worth of real estate. Fewer still—not more than two dozen families—accumulated enough property to be considered affluent. Unlike the lower states, free blacks in the upper region had been part of a large-scale manumission during the 1790s and early 1800s. They entered freedom with few of the resources and skills required to accumulate significant amounts of wealth. Largely black, representing a much larger proportion of the total population of color (by 1840, 12.5 percent compared with 3.1 percent in the Lower South), and much more closely tied to the slave than to the white population, most free families of color in the Upper South had a difficult struggle to survive, much less to become independent landholders and property owners. As the free black population expanded from 30,158 in 1790 to 94,085 in 1810 to 174,357 by 1840 (compared with 2,199 to 14,180 to 41,218 in the Lower South), free blacks in the Upper South were forced to compete with one another as well as with native whites, immigrants, and slaves for even the most menial jobs. By 1840, four out of five free blacks in the South resided in the Upper South; among them, a significant portion had been born in slavery or had slave parents.²¹

The few free blacks who did accumulate several thousand dollars worth of real estate were similar to their counterparts in the Deep South: they were most likely to be of mixed racial origin; they were often the children or grandchildren of whites; they usually received some assistance from white benefactors. The two wealthiest farmers in Virginia, Priscilla Ivey and Frankey Miles, were mistresses of white slaveowners. Two others, Francis and Alfred Anderson, were children of a white planter and his slave. North Carolina barber-planter John Carruthers Stanly was the son of the prominent merchant-shipper John Wright Stanly and an African-born Ibo woman. Several other prosperous North Carolina free blacks—cabinet maker Thomas Day, merchant Louis Sheridan, and contractor James D. Sampson—had similar mixed racial backgrounds, while slave-born Robert Rentfro, a hotel owner in Nashville, was emancipated and given a bequest by his white owner. With its early Spanish, French, and Creole traditions, St. Louis was similar in some respects to the Lower Mississippi region, and the four most prosperous free families of color during this early period—the Clamorgans, Labadies, Mordecais, and Rutgers—were all descendants of early white settlers and black women. Louis Rutgers, who eventually amassed an estate of \$50,000, was the slave son of Dutch immigrant Arend Rutgers, who had received a land grant from the French government a few years before the Louisiana Purchase.²²

1822), 84–85; Olmsted, *Cotton Kingdom*, 132; Charles O. Screven to Anthony Odin[g]sells, July 30, 1828, in Joseph Parsons, "Anthony Odinsells: A Romance of Little Wassaw," *Georgia Historical Quarterly*, 55 (Summer 1971): 210; John William De Forest to wife, September 29, 1862, in James H. Croushore, ed., *A Volunteer's Adventures: A Union Captain's Records of the Civil War* (New Haven, Conn., 1946), 47–48.

²¹ Berlin, *Slaves without Masters*, 46–47, 136–37.

²² Jackson, *Free Negro Labor and Property Holding*, 123, 127, 217; Deed of Emancipation, David Ross to Christopher MacPherson, June 2, 1792, in Legislative Records, Petition of Christopher MacPherson to the Virginia General Assembly, December 10, 1810, Richmond City, Virginia State Library, Richmond, Va.; RCPC, Craven Co., N.C., Petition of Alexander Steward to Emancipate the Slave John, March 12, 1795, in Slave and Free Negro File, North Carolina Division of Archives and History, Raleigh, N.C.; Willard Gatewood, "'To Be Truly free': Louis Sheridan and the Colonization of Liberia," *Civil War History*, 29 (December 1983): 332; Louis Sheridan to Joseph Gales, May 20, 27; June 1, 17; July 22, August 8, 1836, January 6, 13, 1837, in Records of the American Colonization Society, reels 26–27, Library of Congress, Washington, D.C.; Julius Melbourn, *Life and Opinions of Julius Melbourn* (Syracuse, N.Y., 1847), 7–8, 11, 56–57; Legislative Records, Petition of [the] Inhabitants of Davidson County to the Tennessee General Assembly, no. 20–1–1801, Tennessee State Library and Archives, Nashville, Tenn.;

Yet, as was the case in the Lower South, those who received such assistance would not have been able to maintain their economic standing without a shrewd understanding of business affairs. Some benefited from rising property values or periods of general prosperity, but others aggressively expanded their holdings. In 1829, Joseph Jenkins Roberts and William Nelson Colson of Petersburg, Virginia, established a business partnership to trade in West Africa. They secured a schooner, obtained credit in Philadelphia and New York, and began purchasing finished goods to sell and trade for ivory, wood, and palm oil. Even as a slave, New Bern's John Carruthers Stanly, known as "Barber Jack," had acquired considerable real and personal property. Once free, he built up a large clientele, turned his barber shop over to his two most trusted slaves, and began purchasing rental houses and plantation acreage. Using similarly aggressive tactics, other Upper South farmers, merchants, cattle dealers, and business people expanded their property holdings during these early years.²³

Although black slaveholding developed quite differently in the upper states—in 1830, only one free black family in fourteen owned slaves, most of whom were members of their own families or loved ones—some who reached the upper levels of wealth, like their counterparts in the lower states, acquired gangs of black laborers. Virginia farmer Priscilla Ivey, North Carolina merchant Louis Sheridan, and Tennessee farmer Sherod Bryant bought, sold, mortgaged, and hired black workers on a regular basis. During the period 1820–1828, John Carruthers Stanly, despite his own slave heritage, increased the size of his slave labor force to 163 blacks (the largest total for a free black in the South at the time). A regular bidder at local auctions, he primarily bought young men to work on his cotton and turpentine plantations. One New Bern resident later recalled that Stanly was a "hard task-master" who demanded long hours in the field and "fed and clothed indifferently." By the late 1820s, Stanly had become one of the leading slaveholders and wealthiest property owners in his section of North Carolina. His total estate exceeded \$68,000.²⁴

But Stanly was highly unusual. During this early period, most of the wealthiest free blacks in the region owned only a few slaves or had not entered the slaveholding class. They also controlled relatively modest amounts of real estate. In 1825, the richest black in the District of Columbia, Charles King, owned real estate assessed at only \$4,088, while in other towns and cities of the border region—Baltimore, Richmond, Petersburg, Lexington, Louisville, Nashville—free black

Anita Goodstein, "Black History on the Nashville Frontier, 1780–1810," *Tennessee Historical Quarterly*, 30 (Winter 1979): 412–13; RCPC, St. Louis, Mo., Deeds, bk. M (December 6, 1825), 421; *ibid.*, Estates, no. 637, July 29, 1825; Estates, no. 6301, March 2, 1863; Estates, no. 769, October 4, 1859; Lawrence Christensen, "Cyprian Clamorgan, the Colored Aristocracy of St. Louis (1858)," *Bulletin of the Missouri Historical Society*, 31 (October 1974): 5, 6, 13, 14, 16, 22; Schweninger, *From Tennessee Slave to St. Louis Entrepreneur*, 102–03.

²³ Luther Porter Jackson, "Free Negroes of Petersburg, Virginia," *Journal of Negro History*, 12 (July 1927): 376; William Colson to Joseph Gales, July 24, September 16, 1834, Records of the American Colonization Society; RCPC, Craven Co., N.C., Deeds, bk. 33 (January 10, 1800), 495; Deeds, bk. 38 (April 10, 1811), 102; Deeds, bk. 39 (July 1, 1815), 503; *ibid.*, St. Louis, Mo., Deeds, bk. S3 (July 25, 1835), 304.

²⁴ Jackson, *Free Negro Labor and Property Holding*, 122, 127, 205–06, 216, 224; Schwarz, "Emancipators, Protectors, and Anomalies," 323–24; Woodson, *Free Negro Owners of Slaves in the United States in 1830*, vi; Halliburton, "Free Black Owners of Slaves," 135, 142; J. Merton England, "The Free Negro in Ante-Bellum Tennessee," *Journal of Southern History*, 9 (February 1943): 54n; RCPC, Craven Co., N.C., Deeds, bk. 45 (May 8, 1828), 440–45. The dollar amount of John Stanly's total estate is an estimate derived from various deeds. New Bern resident John D. Whitford's recollection is found in the *Raleigh Morning Post*, December 5, 1897.

coopers, blacksmiths, shoemakers, and grocers controlled similarly small estates. In 1842, the largest black property owners in Frankfort, Kentucky, included grocer John Ward, who owned a house and lot worth \$4,000; plasterer Harry Mordicai, who owned a house worth \$3,000; and drayman Tom Bacon, who was worth \$2,500. The same was true in rural areas. The largest free black farmers in Delaware, Maryland, Virginia, Kentucky, and Tennessee owned land valued at between \$3,000 and \$7,000. The wealthiest black farmer in Virginia, William Jarvis, for example, left an estate valued at \$6,656 in 1825, while his counterparts in other states, who usually worked the land themselves, were classified only as “middle-sized” farmers. In short, by the late 1830s, few free blacks in the Upper South could match the substantial wealth holdings among free people of color in the lower states, and even the most prosperous often possessed comparatively small estates.²⁵

During the 1840s and 1850s, the profile of prosperous blacks in the Upper South changed dramatically. A general upturn in the economy from the mid-1840s onward—improved roads and river transportation, rising industrial development, new techniques for replenishing the land, higher wages, and growth in the demand for skilled workers—was partly responsible, but it was not only the general prosperity that caused the number of prosperous blacks to rise more than 100 percent during the 1840s and nearly 200 percent during the next decade. Nor was this change reflected in the small (29 percent) increase in the free black population. A generation after their parents were in bondage, free blacks had acquired a considerable degree of experience in dealing with economic matters. Some shrewdly began purchasing land when prices were low and either sold or improved their holdings as land values rose. Others gradually added to their realty holdings. In towns and cities, they purchased lots, unimproved land, buildings, and business property; in rural areas, they expanded their livestock herds, diversified their crops, and increased their farm acreage. Although a few entered more highly skilled occupations or started new businesses, most continued their same farming or business operations but with a better understanding of how to expand their holdings.

Those who reached the upper levels of wealth during this period had usually spent many years expanding their estates. Baltimore barber Thomas Green, who had arrived from Barbados as early as 1813, opened a small barber shop on Light Street. During the 1830s and 1840s, he purchased seven rental houses, several vacant lots, and saved his extra earnings. By 1858, at the age of seventy-one, his estate—the largest for any free black in the state—stood at \$17,139, including \$5,923 in cash. While not as wealthy as Green, Virginia farmer William Epps made a similarly remarkable economic ascent during the late antebellum era. Beginning with virtually nothing in the 1820s, Epps gradually purchased real estate and added to his personal holdings, until, by 1850, he was listed in the census as a “planter.” He owned no slaves, but in 1860 his \$7,700 worth of property made him one of the most successful black farmers in the Upper South. Virginia bondsman John Berry Meachum purchased his own freedom, moved to Kentucky, and then to St. Louis,

²⁵ Letitia Woods Brown, *Free Negroes in the District of Columbia, 1790–1846* (New York, 1972), 139, 152–55; Jackson, *Free Negro Labor and Property Holding*, 122, 127–28, 144–47, 321–22; “Pursuits &c of the Free People of Color in the Town of Frankfort,” July 16, 1842, Filson Club, Lexington, Ky.; RCPC, Davidson Co., Tenn., Wills and Inventories, vol. 9 (July 7, 1832), 596; *ibid.*, Deeds, bk. 2 (March 12, 1840), 389–90; J. Merton England, “The Free Negro in Ante-bellum Tennessee” (Ph.D. dissertation, Vanderbilt University, 1941), 49.

arriving there in 1815 with a few dollars in his pocket. Within a few years, he had opened a barrel-making establishment and begun buying real estate. At mid-century, he owned two brick buildings in St. Louis and a farm in Illinois. His \$8,000 in real estate placed him among the three largest black realty owners in the state of Missouri.²⁶

As the careers of Green, Epps, and Meachum suggest, those who reached the upper economic levels in the border states during the 1840s and 1850s were quite different in background, occupational status, and sources of wealth from affluent free people of color in the Deep South. At mid-century, according to the census, Epps was the only "planter" (among those with at least \$2,000 in realty) in the entire Upper South. A few farmers owned small work gangs of slaves, but they represented only a tiny segment among the most prosperous free blacks. Indeed, by 1860, in Delaware, Maryland, the District of Columbia, Virginia, and Kentucky, the phenomenon of free blacks owning slaves for profit had nearly disappeared. Among those with more than \$2,000 in realty in 1850, there were three times as many "laborers" as merchants. Next to "farmer," the largest single occupational category was barber: about 12.5 percent of the black economic elite owned barbershops. There was 1 grocer, 1 grain dealer, 4 "storeowners," and 3 livery owners; 17 prosperous free blacks worked as blacksmiths, bricklayers, carpenters, or shoemakers; 22 others labored as cooks, gardeners, painters, plasterers, laborers, porters, or stewards. Meachum was the only cooper listed as having large wealth holdings, and there was only 1 mechanic. By the eve of the Civil War, unskilled and semi-skilled workers still outnumbered store owners and skilled artisans, and farm owners and barbers still constituted more than half of the most affluent class.²⁷

SHARP CONTRASTS BETWEEN PROSPEROUS FREE PEOPLE OF COLOR in the Upper and Lower South existed in other areas besides occupational structure. At mid-century, nearly half of the most affluent group in the upper states was listed in the census as "black" (49.3 percent) as opposed to "mulatto," compared with about one out of ten (9.4 percent) in the lower states. While this was partly a reflection of the proportion of blacks versus mulattoes in the free black population in the two sections, the economic elite in the Upper South more nearly mirrored the free black population. At the same time, only a small proportion of the economic upper class was female (13 percent), often women who had purchased themselves out of

²⁶ *Condition of the Colored Population in the City of Baltimore* (n.p., 1838), 7, in Moorland-Spingarn Research Center, Howard University, Washington, D.C.; RCPC, Baltimore, Md., Chancery Papers, 1858, Accession no. MdHr 40200-5988-1/2, Maryland State Archives, Annapolis, Md.; *Baltimore American and Commercial Advertiser*, May 18, 1864; *New York Herald*, April 1, 8, 1863; Spraggins, "History of the Negro in Business," 47; James Wright, *The Free Negro in Maryland, 1634-1860* (New York, 1921; rpt. edn., New York, 1971), 192. Wright's estimate of Green's total estate differs slightly from the inventory in his estate papers. USMSPC, Baltimore, Md., 9th Ward, 1850, 47; *ibid.*, Halifax Co., Va., 1850, 95; 1860, 877; *ibid.*, St. Louis, Mo., 4th Ward, 1850, 49; RCPC, St. Louis, Mo., Estates, no. 4173, April 12, 1854; Donnie Bellamy, "Free Blacks in Antebellum Missouri, 1820-1860," *Missouri Historical Review*, 67 (January 1973): 216, 224.

²⁷ Computed from USMSPC, 1850, 1860. Statistical percentages on occupations are the *valid percentages*, that is, the percentage of those whose occupations were listed. In 1850, census takers listed the occupations for 79 percent of the prosperous blacks in the Upper South; in 1860, this had risen to 89 percent. USMSPC, Baltimore, Md., 9th Ward, 1850, 47; *ibid.*, Henrico Co., Va., Richmond, 1850, 248; *ibid.*, Jefferson Co., Ky., Louisville, 4th Ward, 1850, 50; *ibid.*, Davidson Co., Tenn., Nashville, 3d Ward, 1850, 356; *ibid.*, Shelby Co., Tenn., Memphis, 7th Ward, 1850, 185; *ibid.*, St. Louis, Mo., 2d Ward, 1850, 222; 3d Ward, 352; 4th Ward, 54-55; Herbert A. Thomas, Jr., "Victims of Circumstance: Negroes in a Southern Town, 1865-1880," *Register of the Kentucky Historical Society*, 71 (July 1973): 268.

slavery and acquired tracts of land over many years. In contrast, more than one-third of the most prosperous group in the Deep South (36 percent) were free women of color, often mulatto women who had lived with white men or inherited estates from white merchants or planters. In both Upper and Lower South, the group was highly urbanized, but in the upper states prosperous free blacks were more likely to live in towns and cities (compared with the general urban-rural configuration of the population) than their counterparts in the lower states. About one-third of the free black population in 1850 lived in urban areas, while 53 percent of the economic elite lived in these areas. In the lower states, more than 50 percent of the free black population was urban, and 65 percent of the upper-wealth group lived in towns and cities.²⁸

Although only a tiny number of free black families in the upper states accumulated at least \$2,000 worth of real estate—one out of seventy-three in 1860, compared with one out of ten in the Lower South—during the 1850s, prosperous free blacks in the border states made considerable advances compared with their counterparts in the lower tier of states. Their numbers nearly tripled, from 213 to 619; they increased their total real estate wealth from \$845,000 to \$2,537,100; they slightly increased the average value of their holdings from \$3,969 to \$4,099. Most of those who entered the upper-level group during this decade owned relatively modest amounts of real estate or increased their holdings by only a few thousand dollars, but emerging in the Upper South was a group of truly wealthy free blacks—Baltimore caterer Henry Jakes, Georgetown livery stable owner and feed merchant Alfred Lee, North Carolina merchant-farmer Hardy Bell, Tennessee gardener Lewis Doxey, and St. Louis tobacconist William Deaderick—who owned between \$8,000 and \$50,000 worth of real estate. Despite limited opportunities, they made remarkable economic strides during the last decade before the Civil War.²⁹

They did so without forming exclusive social and cultural clans. The tiny number of prosperous free blacks, their wide geographical dispersal, the lack of opportunity in higher paying jobs, and the largely black compared with mulatto populations resulted in close ties with other groups of blacks—less affluent property owners, propertyless free blacks, and slaves. It was not uncommon for affluent free black and slave families to be interrelated, and it was practically impossible (except perhaps in St. Louis), given the small number at the top of the economic hierarchy and their rather modest means, for parents to insist that their children marry into families of similar economic circumstances. Consequently, prosperous free blacks in the Upper South mingled with other blacks—slave and free—at tipping houses,

²⁸ Computed from USMSPC, 1850; calculated from U.S. Census Office, *The Seventh Census of the United States: 1850* (Washington, D.C., 1853), *passim*; *Statistical View of the United States . . . Being a Compendium of the Seventh Census* (Washington, D.C., 1854), 83; Berlin, *Slaves without Masters*, 136–37. At mid-century, the proportion of “blacks” in the free black population in the upper states was 61 percent and in the Lower South, 31 percent. Historians have long been aware of the problems of analyzing “black” and “mulatto” designations in the census returns, but the data do provide rough gauges for analyzing mixed racial ancestry. See Robert Brent Toplin, “Between Black and White: Attitudes towards Southern Mulattoes, 1830–1861,” *Journal of Southern History*, 45 (May 1979): 185–200.

²⁹ USMSPC, Baltimore, Co., Md., 1st Dist., 1850, 226; 4th Dist., 1860, 147; *ibid.*, Dorchester Co., Md., 1850, 421; *ibid.*, 9th Dist., 1860, 1019; *ibid.*, Alexandria Co., Va., Alexandria, 1860, 875; *ibid.*, Jefferson Co., Ky., Louisville, 1st Ward, 1860, 74; *ibid.*, Davidson Co., Tenn., Nashville, 1850, 94; *ibid.*, 6th Ward, 1860, 437; *ibid.*, Baltimore, Md., 11th Ward, 1860, 560; *ibid.*, District of Columbia, Georgetown, 1850, 186; *ibid.*, Georgetown, 1st Ward, 1860, 3; Henry Robinson, “Some Aspects of the Free Negro Population of Washington, D.C., 1800–1862,” *Maryland Historical Magazine*, 64 (Spring 1969): 52; USMSPC, Warren Co., N.C., 1860, 279; *ibid.*, Davidson Co., Tenn., 13th Dist., 1860, 225; *ibid.*, St. Louis, Mo., 7th Ward, 1860, 113.

TABLE 2
Real Estate Holdings among Prosperous Free People of Color in the
Upper South, 1850 and 1860 (those with at least \$2,000)

State	Owners		Average Real Estate Holdings	
	1850	1860	1850	1860
Delaware	13	38	\$5,000	\$3,076
District of Columbia	8	89	2,375	3,082
Kentucky	41	71	3,668	3,875
Maryland	53	167	3,857	3,825
Missouri	15	65	6,407	6,515
North Carolina	14	32	4,329	5,538
Tennessee	21	56	3,719	4,780
Virginia	48	101	3,581	3,601
Total	213	619	\$3,969	\$4,099

SOURCE: Computed from the USMSPC, 1850, 1860.

barbecues, sporting events, and slave dances. They attended the same churches, joined the same benevolent organizations, and whenever possible, enrolled their children in the same schools.³⁰

These demographic and cultural differences were also reflected in race relations. In both regions, of course, free blacks were confronted with repressive laws, periodic outbreaks of racial violence, and white hostility, but the close paternalistic ties of South Carolina and the Gulf region were largely absent in the border states. Whites in the upper states did provide "protection" to individual free blacks whom they deemed especially industrious, but rarely did they defend free blacks as a group. Nor did they believe that free blacks would side with whites in case of slave unrest or insurrection. Rather, they tended to view free blacks, including the most prosperous, as setting a bad example for slaves. Thus, while prominent South Carolina and Louisiana whites defended free people of color as "an industrious and honest people," slaveholders and planters in the border region, even those who allowed some of their slaves virtual freedom, castigated free blacks as "indolent," "thieving," "ungovernable," and "depraved."³¹ By the late antebellum era, in nearly

³⁰ Schweningen, *From Tennessee Slave to St. Louis Entrepreneur*, 6; Wright, *Free Negro in Maryland*, 250–51; Legislative Records, Petition of the Members of the African Benevolent Association to the Senate and House of Representatives of Delaware, January 1825, Delaware Hall of Records, Dover, Del.; Jackson, *Free Negro Labor and Property Holding*, 163n; Willard B. Gatewood, Jr., ed., *Free Man of Color: The Autobiography of Willis Augustus Hodges* (Knoxville, Tenn., 1982), xxiv, xxviii, 23, 25–26; RCPC, Alexandria Co., Va., Wills, vol. 1821–31 (November 25, 1829), 342, in MESDA; Luther Porter Jackson, "Religious Development of the Negro in Virginia from 1760 to 1860," *Journal of Negro History*, 16 (April 1931): 193–98.

³¹ Legislative Records, Petition of the Citizens of King William County to the Virginia General Assembly, January 20, 1842, Virginia State Library; *ibid.*, Petition of the Citizens of Hardeman County to the Tennessee Senate and House of Representatives, December 3, 1857, no. 94, Tennessee Department of Archives and History, Nashville, Tenn.; Barbara Jeanne Fields, *Slavery and Freedom on the Middle Ground: Maryland during the Nineteenth Century* (New Haven, Conn., 1985), chap. 4. These generalizations are based on an analysis of approximately 1,595 petitions to state legislatures concerning race, slavery, and free blacks between 1777 and 1866. The states with the largest number of extant petitions (excluding those that were redundant or repetitive)—Virginia (425), North Carolina (154), South Carolina (599), Mississippi (102), Texas (62), and Tennessee (221)—are equally divided between the Upper and Lower South and give a clear picture of the different attitudes toward free people of color in the two sections.

every respect—average wealth, color, gender, residency, family attitudes, social and cultural values, and relations with the dominant race—prosperous free blacks in the Upper and Lower South lived as if in two different worlds.

JUST AS THE CIVIL WAR profoundly altered the status of former slaves, it also created a new environment for formerly free people of color. In the Lower South, the war and its aftermath spelled disaster for the great majority of free blacks who had, only a short time before, been among the most prosperous blacks in the nation. “When [the] war commence it [was] purty hard on folks,” a free black in St. Mary Parish, Louisiana, recalled. First came the Confederates, who swept up the slaves, including those owned by blacks, and took them away to build fortifications; then came the Yankee raiding parties, who rode through the countryside, burning, looting, and pillaging. “The road all the way to Natchitoches,” one observer said, describing the area where some of the wealthiest free people of color in the South owned their plantations, “was a solid flame.”³²

Following the war, the great majority of formerly free black slaveholders were unable to rebuild their antebellum fortunes. After three successive crop failures, South Carolina rice planter Robert Collins, who had once owned a 3,100-acre plantation and seventeen slaves, was forced to borrow money from the Freedmen’s Bureau to purchase supplies for his former bondpeople. In Alabama, Mississippi, and Texas, the Collins, Barland, and Ashworth clans experienced the same difficulties, while Louisiana’s Creoles of color not only lost their slaves, farm machinery, livestock, buildings, and personal possessions but also much of their land. Among those forced to sell their estates for non-payment of debts were St. Landry’s Josephine Decuir, who, along with her husband, had owned 112 slaves and had been worth approximately \$160,000; Plaquemines’s Andrew Durnford, Jr., whose father had been among the twelve wealthiest blacks in America at the time of his death in 1859; and St. Mary’s Romaine Verdun, who had once overseen a large, highly profitable plantation.³³

Statistical evidence reveals the extent of this decline. Fewer than one out of five (approximately 194 of 1,121) prosperous antebellum free blacks in the Lower South survived the war as realty owners, a persistence rate substantially lower than that for whites in the war-ravaged black belt of Alabama. Among the 194, 66 had dropped below the \$2,000 real estate level and 128 continued to hold at least that amount. During the postwar years, the new entry rate into the \$2,000-plus group also fell off sharply. Among antebellum free blacks who had held land and other property for at least a decade, the entry rate into the most prosperous group went from 131 in 1850 to 147 in 1860 to only 69 in 1870. The average realty controlled by the new entrants dropped in value from \$9,100 in 1850 to \$6,200 in 1860 to \$4,300 in 1870. Among free blacks who had entered the upper levels of wealth in 1860 and continued to be property owners after the war, including four who had

³² George Rawick, ed., *The American Slave: A Composite Autobiography*, 19 vols. (Westport, Conn., 1972), 5: pt. 4, p. 158; quoted in Mills, *Forgotten People*, 237.

³³ Petition for Relief of Antoine Meullion, December 1889, in Meullion Family Papers; Koger, *Black Slaveowners*, 120–24, 193–95; RPPC, Iberville Parish, La., Deeds, bk. 9 (July 15, 1868), 221–23; USMSPC, Pointe Coupée Parish, La., Successions, no. 203, July 11, 1865; *ibid.*, Natchitoches Parish, La., Conveyances, vol. 69 (December 20, 1873), 601–04, 637–39; *ibid.*, St. Mary Parish, La., 1870, 574; Loren Schwenger, “Antebellum Free Persons of Color in Postbellum Louisiana,” *Louisiana History*, 30 (Fall 1989): 345–64.

migrated from the Upper South, more than one out of three (58 of 151) had dropped below the \$2,000 level. The average real estate holdings of this latter group stood at less than \$800. By 1870, the affluent class in Louisiana and South Carolina, which had previously controlled \$5,632,100 worth of real estate, 44 percent of the black-owned property in the entire South, owned land worth only \$3,851,100, slightly less than 12 percent of the total holdings.³⁴

Even those who survived the war with their estates intact—primarily, urban free blacks who had invested heavily in real estate rather than slaves—found it difficult to adjust to the rapid changes occurring in the wake of emancipation. Saddened by the passing of the old regime, disheartened by the loss of their privileged status, angered at being mistaken for former bondsmen, they ignored their business obligations during the 1870s and allowed their real estate holdings to evaporate. Three of the wealthiest antebellum free people of color in New Orleans, for example, became so despondent during the postwar era that they committed suicide. While such extreme responses were rare, in Charleston, Savannah, Mobile, Natchez, Baton Rouge, and other towns and cities, those who had once been among the largest property owners quickly passed from the scene through death or the mismanagement of their holdings during a period of economic chaos.³⁵

With the rapid decline of their economic fortunes came the disintegration of the pre-war clans of free blacks. Members of various families drifted apart or, as was the case for the Verdun and Metoyer families in Louisiana, subsisted on small plots of their once-great plantations. As former slaveowners, landowners, and respected "citizens" in their communities, they longed to return to the past. Among them, perhaps no one expressed their feelings more poignantly than Catherine Johnson, the eldest daughter of the wealthy Natchez, Mississippi, barber William Johnson, who had been brutally murdered in 1851 during a boundary dispute. Part of her despair was personal—the lingering memories of her father, the growing insanity of her brother, the financial difficulties of her mother—but these problems seemed to be symptomatic of a deeper anguish, an anguish caused by the incomprehensible changes taking place as a result of the war. "[T]o the present the past seems so Bright, so bright that I dare not call up its memories, for it makes me wretched to think that in reality I can never live them again," she despaired in 1866, "and I know that it is wrong but sometimes I do long to die."³⁶

The Civil War also transformed the antebellum pattern of race relations in the

³⁴ The persistence rate has been calculated as follows. The number of individual property owners in 1850 and 1860 who attained \$2,000 in realty (1289 minus 168 who appeared in both census years = 1121) was divided by the number listed in 1870 who also appeared in 1850 and/or 1860, or 194 "repeats." Thus 194 divided by 1121 = .17, or 17 percent. The same procedure was used for the Upper South. The decline in realty holdings should be put in the context of declining postwar land values. In rural areas of the Deep South, the value of land dropped between 55 and 70 percent after the war. In the South as a whole, it declined 45 percent in real terms. In 1850 and 1860, in both sections of the South, a large segment of the most prosperous group was aged fifty and over. In the Lower South, in 1850, the fifty-plus age group constituted 41 percent of the elite property owners, and in 1860, 48 percent; in the upper states, in 1850 and 1860, 49 percent. A significant portion of these property owners died of natural causes during the 1860s. For comparative purposes, however, since the mean age—late forties—and proportion above age fifty were so similar in two sections, the death rate would have had a negligible effect on the comparative persistence rates in the two sections. See Roger L. Ransom and Richard Sutch, "Capitalists without Capital: The Burden of Slavery and the Impact of Emancipation," *Agricultural History*, 62 (Summer 1988): 133–60; Ransom and Sutch, *One Kind of Freedom*, 51; Soltow, *Men and Wealth*, 64.

³⁵ RPPC, New Orleans, La., Successions, no. 38,677, May 27, 1876, in New Orleans Public Library; Rankin, "Impact of the Civil War," 403–06; USMSPC, New Orleans, La., 6th Ward, 1870, 235.

³⁶ Diary of Catherine Johnson, May 30, 1866, in William Johnson Papers, Library Manuscript Department, Louisiana State University, Baton Rouge, La.

Deep South. The old paternalism, which had governed the relations between prosperous free people of color and whites, rapidly changed. The worsening economic condition of former planters—both white and black—the racial tensions created by Reconstruction, the entry of formerly free people of color into the political arena, the ideological differences between whites and blacks over the Fourteenth (citizenship) and Fifteenth (franchise) amendments pushed formerly aristocratic whites and formerly free blacks away from one another. Of course, there were ideological and political differences among ex-free black business people and planters, but most of them realized that, as one Alabama Creole of color who had formerly held himself aloof from blacks said, “our future is indissolubly bound up with that of the Negro race.” As blacks moved into the Republican party, the party of Lincoln, their former “protectors,” who had previously signed petitions in their behalf, joined the Democrats. Whites in the Deep South could only lament that formerly prosperous free people of color proved to be nearly as “disloyal” as their former slaves.³⁷

As the position of property-owning free people of color in the Deep South declined, a new black economic elite began to emerge. Since census takers did not ask questions about former status (“Were you born a slave?” “Did you gain your freedom prior to the Civil War?”), only the vague outlines of this new elite can be discerned, but a number of indexes show that, as the antebellum group so closely tied to the white aristocracy lost economic status, former slaves and formerly propertyless free blacks emerged as leading property owners. For example, the total number of Deep South blacks who owned at least \$2,000 worth of real estate rose 81 percent between 1860 and 1870 (706 to 1,278). Before the war, 86 percent of the most prosperous group was mulatto and 32 percent female; during the postwar era, these two indexes had dropped to 56 and 19 percent, respectively. The mean realty holdings among mulattoes dropped from \$9,082 in 1860 to \$5,448 in 1870, but among blacks, who now comprised 44 percent of the upper-wealth group, it rose from \$4,056 to \$4,367.

NEARLY AS PRECIPITOUS AS the decline of formerly free people of color in the Lower South was the rise of free blacks in the Upper South during the Civil War decade. In contrast to the lower states, Upper South black slaveholders had owned few slaves; except in Virginia and North Carolina, they were often far removed from the fields of battle, and they did not experience the same severe depreciation in the value of their pre-war holdings as did free people of color in the lower states. Indeed, in some areas, especially the border towns and cities, property values actually rose during the 1860s. Formerly free blacks in the upper states also benefited from an increasing demand for skilled and semi-skilled workers, less fear by whites of “Negro domination,” at least compared to the regions densely populated by blacks in the lower states, and the short duration of political reconstruction, which in the lower states was marked by prolonged periods of racial strife and violence.

Their ability to improve their economic position varied in different sections of

³⁷ Whitelaw Reid, *After the War: A Tour of the Southern States, 1865–1866* (New York, 1866); rpt. edn., ed. by C. Vann Woodward (New York, 1965), 244; *Mobile Nationalist*, July 11, 1867; Adolphe Garrigues to Charles F. Benjamin, January 28, 1876, Records of the [Southern] Claims Commission, Records of the Treasury Department, Record Group 56, National Archives, Washington, D.C.

TABLE 3
A Profile of Prosperous Blacks in the Lower South, 1860–1870
(those with at least \$2,000 in real estate)

State	Number		Percent						ARPH*	
			Mulatto		Female		Urban			
Alabama	32	149	91	46	25	13	47	48	\$ 3,691	\$4,401
Arkansas		61		41		7		38		4,641
Florida	8	22	100	36	50	14	50	36	3,825	4,977
Georgia	13	127	92	43	46	19	92	47	3,585	3,624
Louisiana	472	510	85	79	31	27	63	57	10,311	5,730
Mississippi	13	162	77	24	31	15	38	27	4,685	5,335
South Carolina	162	206	87	48	32	12	81	56	4,723	4,508
Texas	6	41	83	41	33	10	17	24	5,133	3,134
	706	1,278	86	56	32	19	66	49	\$ 8,384	\$4,971

* = Average Real Property Holdings

SOURCE: Computed from USMSPC, 1860, 1870.

the upper states, in rural and urban areas, and among occupational groups, but nearly everywhere, blacks who acquired at least \$2,000 worth of real estate enjoyed substantial increases in their wealth. In rural Kentucky, Tennessee, and Missouri, where wartime destruction had been slight and blacks represented a relatively small portion of the total population, there was a strong surge into the upper levels of wealth. Although only 37 percent of the Upper South's postwar rural blacks resided there, these three states boasted nearly half of the prosperous farmers in 1870. A significant number among them—including Dennis Lane, John Shaw, Alfred Ewell of Kentucky, Rubin Caldwell, Michael Archer, and Fleming Higgins of Tennessee, Moses Logan, Lewis Mitchell, and Samuel Smith of Missouri—had been free before the war.³⁸

In urban areas, those who had previously carved out a niche in the local economies as artisans, draymen, livery operators, stewards, and barbers, or who had managed small businesses, were often able to expand their operations. Opportunities in the professions and government service improved. In addition, the rapid urban population growth stimulated new demands for service-related employment. As in the rural western states, a significant proportion of those who controlled at least \$2,000 in real estate in the Upper South's towns and cities were formerly free blacks—Wilmington's Charles Agnes, listed as a laborer; Baltimore's Augustus Roberts, a barber; District of Columbia's William Cole, a coachman; Alexandria, Virginia's William Gray, a butcher; Richmond's James Woodson, a boilermaker; Shelbyville, Kentucky's Thomas Ballard, a merchant; and St. Louis's James Young, a steward. Typical of this group was Chestertown, Maryland, restaurant owner William Perkins, who increased the value of his property holdings from \$2,300 to \$5,500 between 1860 and 1867 and, by 1870, after establishing a

³⁸ Computed from USMSPC, 1860, 1870; *ibid.*, Mason Co., Ky., 1860, n.p.; 1870, 518; *ibid.*, Hancock Co., Ky., 1870, 212; *ibid.*, Braken Co., Ky., 1860, 221; 1870, 276; *ibid.*, Greene Co., Tenn., 1860, 368; 1870, 364; *ibid.*, Rutherford Co., Tenn., 15th Dist., 1870, 78; *ibid.*, Andrew Co., Mo., 1850, 59; *ibid.*, Andrew Co., Mo., Nodaway Township, 1870, 278; *ibid.*, Clark Co., Mo., Jackson Township, 1860, 780; *ibid.*, Clay Township, 1870, 467; *ibid.*, Carroll Co., Mo., Wakenda Section, 1870, 414.

popular oyster bar and ice cream parlor, again nearly doubled his property accumulations.³⁹

The number of postwar blacks who reached the higher economic level rose twice as fast in the upper states as in the Lower South, but the group's profile changed only slightly, suggesting stronger continuity with the past. The proportion of blacks and females among the most prosperous group remained relatively stable, while the mean holdings rose from \$4,099 to \$4,375. Both the persistence and new entry rate were higher in the upper states than in the Deep South. About one out of three prosperous antebellum free blacks in the region (approximately 197 of 634) survived the war as realty owners. Among them, 61 percent (120 of 197) remained at or above the \$2,000 level. Following the war, nearly five times as many formerly free black property owners in the Upper South, compared with the lower states, entered for the first time the most prosperous group (316 compared with 69); and, even with this large discrepancy, their average realty holdings stood at nearly the same level as their counterparts in the lower states (\$4,000 compared with \$4,300).⁴⁰ In all, by 1870, approximately 1,814 blacks in the region had acquired at least \$2,000 worth of real estate, a threefold rise in a decade and a figure that now exceeded the number of affluent blacks in the Lower South by a substantial margin.

At the same time, the changing profile among the most prosperous group in the region, especially city dwellers, suggests that some of them had recently emerged from slavery. Not only had the number of prosperous urban blacks risen sharply—285 to 978 or 243 percent—but nearly one out of five worked in menial occupations such as waiters, servants, porters, laborers, hack drivers, and laundresses. In addition, the portion of upper-level wealthy individuals listed by census takers as black had risen from 40 percent to 55 percent during the decade. Indeed, by 1870, there were nearly twice as many prosperous urban blacks in the Upper South as affluent blacks and mulattoes combined before the war. The percentage of this group listed as illiterate also rose during the decade, from 33 in 1860 to 43 in 1870 (including those listed as semi-literate). While none of these comparisons determines the pre-war status of postwar affluent blacks absolutely, and while some ex-free blacks could be found in each of the above categories, taken together, they strongly suggest that some members of the upper-wealth group had been antebellum slaves.⁴¹

The South's new population center for affluent blacks, surpassing Charleston and New Orleans, was the District of Columbia. During the postwar years, blacks obtained government clerkships and teaching positions, started small businesses or continued the enterprises they had managed before the war, and entered the professions as doctors, lawyers, and ministers. Profits were to be made in the building trades, as the city witnessed a rapid expansion of its population, which in turn stimulated growth in construction and home renovation. These new demands

³⁹ USMSPC, New Castle Co., Del., Wilmington, 1st Ward, 1860, 699; 1870, 303; *ibid.*, Baltimore, Md., 9th Ward, 1870, 409; *ibid.*, District of Columbia, 7th Ward, 1860, 887; 1870, 466; *ibid.*, Alexandria, Va., 1860, 841; *ibid.*, 5th Ward, 1870, 134; *ibid.*, Henrico Co., Va., Richmond, 1860, n.p.; *ibid.*, Richmond, Clay Ward, 1870, 422; *ibid.*, Shelby Co., Ky., Shelbyville, 1870, 415; *ibid.*, St. Louis, Mo., 9th Ward, 1870, 519; RCPC, Kent Co., Md., Land Deeds, Liber no. JKH-2 (1860), 230; Liber no. JKH-2 (1861), 456; Liber no. JKH-4 (1864), 445; Liber no. JKH-5 (1866), 359, in Maryland State Archives, Annapolis, Md.; USMSPC, Kent Co., Md., Chestertown, 1860, 1006; *ibid.*, Chestertown, 4th Dist., 1870, 172; A. W. Bolenius to the Freedmen's Bureau, May 31, 1866, Records of the Bureau of Refugees, Freedmen, and Abandoned Lands, Maryland, Record Group 105, National Archives. I wish to thank Professor Richard P. Fuke for providing me with information on Perkins.

⁴⁰ Computed from USMSPC, 1850, 1860, 1870.

⁴¹ Computed from the USMSPC, 1860, 1870.

TABLE 4
A Profile of Prosperous Blacks in the Upper South, 1860–1870
(those with at least \$2,000 in real estate)

State	Number		Percent						ARPH*	
			Mulatto		Female		Urban			
Delaware	38	103	21	21	5	10	11	31	\$3,076	\$4,399
District of Columbia	89	350	54	44	15	18	93	97	3,082	4,962
Kentucky	71	259	51	44	13	15	44	60	3,875	4,051
Maryland	167	320	27	28	7	9	17	41	3,825	4,092
Missouri	65	172	65	26	26	12	91	52	6,515	6,100
North Carolina	32	89	91	39	6	15	13	43	5,538	3,238
Tennessee	56	292	61	36	21	9	50	29	4,780	4,011
Virginia	101	229	59	51	25	14	47	47	3,601	3,837
	619	1,814	49	38	15	13	46	54	\$4,099	\$4,375

* = Average Real Property Holdings

SOURCE: Computed from USMSPC, 1860, 1870.

drove real estate prices in the District upward. Free blacks who had acquired realty before or during the war, especially in the 1st Ward, saw a marked appreciation in the value of their holdings. Edward Crusoe, a grocer worth \$2,000 before the war, for example, established a mercantile grain store during the 1860s and increased the value of his realty holdings by 900 percent. While few business people experienced such a dramatic rise, a number doubled or tripled the value of their pre-war holdings. In all, the number of blacks in the District of Columbia with realty of at least \$2,000 (excluding a few listed in rural areas) shot upward 338 percent (from 83 to 339) during the 1860s, and the total value of their holdings jumped from \$261,300 to \$1,701,000, the largest increases in the South.⁴²

Unlike the antebellum period, when all but a few of the South's wealthiest blacks were residents of the Lower South, by 1870 a number of the region's richest blacks lived in urban areas of the Upper South. District of Columbia hotel owner and restaurateur James T. Wormley, who began as a steward and caterer, established one of the finest hotels in Washington in 1871. By then, his holdings exceeded \$87,000. Another hotel owner, slave-born Henry Harding, worth \$35,000 in 1870, lost a large portion of his wealth with the collapse of the Freedmen's Bank in 1874 but recouped much of it in subsequent years. Former bondsman and free black James Thomas, a barber in St. Louis who married Antoinette Rutgers, the daughter of the wealthiest black woman in Missouri, amassed a fortune speculating in real estate. Within five years after the war, Thomas owned nearly two entire blocks of downtown St. Louis, rented out forty-eight apartment units, and controlled real estate as far away as Memphis and Nashville. His real estate was valued at \$150,000, the third largest for a black in the South.⁴³

⁴² USMSPC, District of Columbia, 1st Ward, 1860, 217; 2d Ward, 1870, 231; 1st Ward, 1860, 318; 1870, 7; 2d Ward, 1860, 523; 1870, 268; 1st Ward, 1860, 335; 1870, 53; 2d Ward, 1860, 595; 1870, 227; 1st Ward, 1860, 333; 1870, 9; 1st Ward, 1860, 378; 1870, 67; RCPC, District of Columbia, Estates, no. 6605, March 2, 1895; *ibid.*, no. 3151, August 3, 1888; computed from USMSPC, District of Columbia, 1860, 1870.

⁴³ USMSPC, District of Columbia, 1st Ward, 1870, 1; RCPC, District of Columbia, Estates, no. 1700, October 31, 1884; Rabinowitz, *Race Relations in the Urban South*, 88; USMSPC, Davidson Co., Tenn., Nashville, 5th Ward, 1870, 266; *ibid.*, St. Louis, Mo., 3d Ward, 1870, 196; RCPC, St. Louis, Mo., Deeds, bk. 405 (April 1, 1870), 69; *Tax Book for the Year 1879*; *State of Missouri*, 26–29; *New York Herald Tribune*,

Not only had prosperous Upper South blacks greatly improved their economic standing during the postwar era, but their family and cultural values also made it easier for them to adjust to new conditions. Most of them welcomed the general emancipation as a glorious day when they could be reunited with their loved ones, mingle freely with various groups of blacks, and establish churches, schools, and benevolent associations free from white control. While some of them became involved in politics, they identified more with freedmen than with their counterparts in the lower states. Despite various business activities, William Perkins of Maryland, for example, devoted much of his time to organizing schools, investigating ex-slaves' complaints of unfair treatment by whites, seeking redress for parents from the state's apprenticeship law (which allowed children to be bound out as laborers), and serving as a representative of the Freedmen's Bureau. Like other prosperous blacks in the region, he felt compassion for the masses of recently emancipated slaves.⁴⁴

In race relations, too, formerly free blacks in the upper states discovered that their independent status served them well during the postwar era. Most of those who owned farms or businesses still had contacts with whites or served white clienteles, but they had never felt the same connection with the white planter and slaveholding class as had their counterparts in the Deep South. Formerly free blacks in the upper states therefore were not tied to the fortunes of the white aristocracy. More self-confident, able to mix more easily with former slaves, and viewing the formerly dominant class with suspicion and skepticism, they could more easily build on their past experiences during the postwar era, to advance not only their own cause but the cause of freedmen as well.

Even though these regional differences reveal how property-owning antebellum free blacks adjusted to postwar conditions, the question of how significant numbers of lower-level free blacks and former slaves were able to enter the highest economic group during the postwar era remains. This development seems all the more remarkable since it occurred in the midst of declining land values, the rise of the Ku Klux Klan and other such groups, the refusal of whites to sell land to freedmen and women, and the declining fortunes of the antebellum planter class. The paucity of scholarly works on black enterprise during the middle period makes answers somewhat tentative, but it appears that, ironically, some postwar difficulties may have actually been beneficial to blacks. Improved farmland sold for between \$15 and \$25 per acre before the war; it now sold for between \$2 and \$8 per acre. In addition, during the war, nearly one out of five Southern white males ages thirteen to forty-three had died, and tens of thousands of others had returned home physically disabled or mentally impaired. Free blacks and emancipated slaves also suffered during the war and its aftermath, but the comparative death rates, at least from what we know, paled by comparison. Moreover, with freedom came a new energy, a new enthusiasm for determining one's own fate in life and acquiring an economic stake. Black leaders urged their people to become economically indepen-

July 6, 1871. The richest black in the South was Mississippi's Benjamin Thornton Montgomery, the former slave of Joseph Davis, who owned \$350,000 worth of real and personal property. USMSPC, Warren Co., Miss., 1870, 131; Hermann, *Pursuit of a Dream*, 109–10, 148, 156–57, 160, 182, 201–03; RCPC, Warren Co., Miss., Estates, no. 3029, November 6, 1877.

⁴⁴ A. W. Bolenius to the Freedmen's Bureau, May 31, 1866; William Perkins to E. C. Knowler, May 4, 1868, Records of the Bureau of Refugees, Freedmen, and Abandoned Lands, District of Maryland, Letters Received by Assistant Commissioners, Record Group 105, National Archives.

dent and self-sufficient.⁴⁵ Although none of these conditions necessarily resulted in economic improvement, and the masses remained landless, in every section of the South enterprising blacks achieved a measure of prosperity by acquiring significant amounts of land and property.

During the 1870s, these trends continued. In the Lower South, the small group of free people of color who had owned large plantations and gangs of slaves—once the richest group of blacks in the United States—continued to decline in number and wealth. Even in Charleston and New Orleans, where postwar economic activity quickly revived, the value of the average holdings of blacks who had been at the top of the economic pyramid dropped sharply. With a prolonged depression beginning in 1873, continued violence and political unrest, and another generational change, which witnessed the death and breaking up of the estates of several surviving formerly free people of color, the number of prosperous pre-war blacks remaining as large property owners by the late 1870s could be counted on one hand.⁴⁶

Although affluent free people of color in the Lower South failed to maintain their unique status and economic standing in the postwar era, formerly free blacks and former slaves in the Upper South continued to gain entry into the most prosperous group of farmers, skilled artisans, and small business people. During the 1870s, despite the economic problems of the era, the most prosperous group continued to expand, especially in towns and cities. Compared with whites, who had twenty-two out of every hundred families at this wealth level, this group remained tiny even in the Upper South, with only about .5 percent of the region's black families having acquired such wealth. But remarkable shifts occurred following the war, and prosperous blacks in the upper states had significantly expanded their estates.

THUS, IN THE THREE GENERATIONS beginning in the 1790s and ending in the 1870s, significant changes took place among the most prosperous group of blacks in the South. During the early years, only a tiny number of free people of color, usually light-skinned mulattoes directly related to the slaveowner class, were able to achieve a higher economic standing. Their ranks included a substantial number of women, and two states in the Lower South—South Carolina and Louisiana—contained most of the economic successes. By the 1830s and 1840s, this distribution had begun to change, with the gradual emergence of an affluent group in the Upper South, largely urban and black, often with direct ties to slavery. Prosperous free blacks in the two regions developed different social and cultural values, maintained different relations with the dominant race, and engaged in different occupations. The Civil War accelerated the changes that had begun during the 1840s and 1850s, and, by

⁴⁵ Ransom and Sutch, *One Kind of Freedom*, 82; Maris A. Vinovskis, "Have Social Historians Lost the Civil War? Some Preliminary Demographic Speculations," *Journal of American History*, 76 (June 1989): 38–39; Allen W. Trelease, *White Terror: The Ku Klux Klan Conspiracy and Southern Reconstruction* (London, 1972); Leon F. Litwack, *Been in the Storm So Long: The Aftermath of Slavery* (New York, 1979), 399–408; Howard Rabinowitz, ed., *Southern Black Leaders of the Reconstruction Era* (Urbana, Ill., 1982), 250, 285–90, 300–01, 370, 403.

⁴⁶ RCPC, Charleston, S.C., Estates, no. 243–20, December 26, 1876; no. 230–25, March 25, 1875; no. 250–24, May 4, 1878; no. 243–45, January 11, 1877; no. 220–26, July 8, 1873; no. 277–14, May 22, 1882; RPPC, New Orleans, La., Successions, no. 35,055, December 2, 1871; no. 41,626, November 19, 1879; no. 38,677, May 27, 1876, in New Orleans Public Library; *ibid.*, New Orleans, La., Successions, no. 37,326, July 28, 1874, in Louisiana Papers, 65–2, Moorland-Spingarn Research Center, Howard University, Washington, D.C.; Rankin, "Origins of Black Leadership in New Orleans during Reconstruction," 431–32.

the 1870s, a new black economic elite had emerged in the South, one that had forged its existence on the ruins of a vanquished civilization. Only a decade before this remarkable transformation, some of the most prosperous blacks in the South had been considered “a species of property” themselves.

A Note on the United States Population Censuses

In several sections of this essay, I have relied heavily on the U.S. Manuscript Population Censuses for 1850, 1860, and 1870. During these years, census marshals were required to “insert the value of real estate owned by each individual enumerated” and, from 1860 to 1870, the value of personal holdings including “bonds, mortgages notes, slaves, live stock, plate, jewels, or furniture.” They were to obtain this information in dollars by personal inquiry of each family head. Personal inquiry had its limitations, and, during the violent summer of 1870, census takers probably failed to canvas 6 or 7 percent of the South’s black population, although this was surely not the case for property owners, among the best-known and most stable members of their communities. Despite its limitations, the middle-period censuses are the only primary sources to connect racial identity with property accumulations for the region as a whole. Consequently, they are a remarkable set of documents for understanding the economic changes among prosperous blacks during the middle period of the nineteenth century.

To obtain data on the black economic elite—those with at least \$2,000 worth of real estate—the manuscript censuses for these years were perused and information gathered on each individual in this category—surname, given name, state, county, town or city of residence, age, gender, color, realty holdings, personal property holdings (1860–1870), state of birth, literacy, and the names and ages of those listed in the same household. These data were supplemented by biographical information on prosperous blacks missed by census takers but listed in local probate records, tax assessment records, and reliable secondary works (in 1850 and 1860, approximately 4 percent of the total). In some of the supplementary sources, the color or gender is not indicated; these “missing cases” were excluded in determining the total percentages. In eighteen cases, individuals listed as whites or Indians in one census were listed as mulattoes in the next. Since other evidence suggests that they were persons of mixed-Negro blood, I have included them as mulattoes.

For comparative purposes, I have obtained data on all black property owners listed in the 1850 and the 1860 censuses. The huge increase in the number of census-listed black families between 1860 and 1870 (from approximately 52,000 to 900,000), however, necessitated that I use a sampling procedure for property owners with total estates between \$100 and \$900 in 1870. For this group, data were taken from every twentieth page and its facing page of the manuscript volumes (total = 7,855). These were considered to be 5 percent of the total in 1870, and thus the data on the four antebellum elite blacks found in this group was multiplied by a factor of twenty. From the group with \$1,000 or more total estate holdings in 1870, all property holders were analyzed.

To analyze property owners who appeared in at least two censuses, I placed blacks in “elite categories,” depending on when they first entered the most prosperous group. These

individuals were then traced forward and backward in time. Those who first entered the elite group in 1850 and were found in the 1860 and/or 1870 returns were analyzed with average realty holdings in each decade, and those who entered the group in 1870 were traced backward to 1860 and/or 1850. While these realty owners were an extremely stable group, rarely moving from one state to another, there was some migration between various states. This method not only provides a "persistence rate"—an estimate of how many prosperous antebellum real estate owners survived the war as property owners—but also generates motion from one decade to the next, the ebb and flow of those entering the most prosperous group and those dropping below \$2,000 in real estate.

The Nose, the Lie, and the Duel in the Antebellum South

KENNETH S. GREENBERG

SOMETIMES, WHITE MEN OF THE ANTEBELLUM SOUTH pulled or tweaked one another's noses. Slaves never pulled anyone's nose; neither did white women. Nose pulling was a meaningful act that appeared almost exclusively in the active "vocabulary" of white men. To pull a nose was to communicate a complex set of meanings to an antagonist and an audience. What did the act mean to the men who performed it and witnessed it?¹ The meaning of any act, like the meaning of words or phrases in written or spoken discourse, cannot be understood in isolation from its context. "Vocabulary"—whether written, spoken, or mimed—is always only a small part of a broader system of meaning. For Southern white men, nose pulling was an action embedded in a larger system of signs—a "language" of honor.² We must reconstruct the system in order to understand the meaning of its parts.³

The borders of the antebellum Southern language of honor are difficult to specify. One rough indicator of the geographic and social boundaries of its active "speakers" is the extent of dueling activity and related affairs of honor.⁴ Although

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¹ It is not my intention to suggest that the pulling of a nose is an act that should be analyzed only in terms of its meaning for Southern white men of honor. A full account of nose pulling would describe its meaning in the language systems of male and female slaves, white women, and other contemporary witnesses.

² "Language" as used in this essay does not mean "vernacular language." It is closer to what Michel Foucault means by "discourse," but I hesitate to use the term "discourse" because I do not accept many of Foucault's assumptions about language. Michel Foucault, *The Order of Things: An Archaeology of the Human Sciences* (New York, 1973); see the discussion of Foucault in John E. Toews, "Intellectual History after the Linguistic Turn: The Anatomy of Meaning and the Irreducibility of Experience," *AHR*, 92 (October 1987): 890; see Mark Poster's essay in Dominick LaCapra and Steven L. Klein, eds., *Modern European Intellectual History: Reappraisals and New Perspectives* (Ithaca, N.Y., 1982), 137–52.

³ My approach is informed by a number of works in intellectual history, linguistics, and ethnography. Of greatest importance are Clifford Geertz, *The Interpretation of Cultures* (New York, 1973); Geertz, *Local Knowledge: Further Essays in Interpretive Anthropology* (New York, 1983); Geertz, *Works and Lives: The Anthropologist as Author* (Stanford, Calif., 1988); James Clifford and George E. Marcus, *Writing Culture: The Poetics and Politics of Ethnography* (Berkeley, Calif., 1986); John Higham and Paul K. Conkin, eds., *New Directions in American Intellectual History* (Baltimore, Md., 1979); Joan Wallach Scott, *Gender and the Politics of History* (New York, 1988); Jonathan Culler, *The Pursuit of Signs: Semiotics, Literature, Deconstruction* (Ithaca, N.Y., 1981); Culler, *On Deconstruction Theory and Criticism after Structuralism* (Ithaca, 1982); LaCapra and Klein, *Modern European Intellectual History*; Dominick LaCapra, *Rethinking Intellectual History: Texts, Contexts, Language* (Ithaca, 1983).

⁴ Contemporary studies of Southern honor include Bertram Wyatt-Brown, *Southern Honor: Ethics and Behavior in the Old South* (New York, 1982); Dickson D. Bruce, *Violence and Culture in the Antebellum South* (Austin, Tex., 1979); John Hope Franklin, *The Militant South* (New York, 1956); Steven M. Stowe, "The 'Touchiness' of the Gentleman Planter: The Sense of Esteem and Continuity in the Antebellum South," *Psychohistory Review*, 8 (Winter 1979): 6–17; Stowe, *Intimacy and Power in the Old South: Ritual in the Lives of the Planters* (Baltimore, Md., 1987); William Oliver Stevens, *Pistols at Ten Paces: The Story of the Code of*

dueling was an elite response to insult, it reveals more than just elite rules for speech and behavior. The prominent political leaders, military men, and newspaper editors who dueled did so to avoid shame before a public audience—an audience that clearly understood and accepted the language and values of honor. Many non-elite men in that audience articulated similar understandings of honor when they settled disputes through fist fights, eye gougings, and ear bitings. Hence it is of considerable consequence that, during the early years of the nineteenth century, dueling and other statements of honor almost completely disappeared in the North and yet remained important for white men of the South. Pockets of Southern resistance to these acts existed—especially in evangelical groups that detected a lack of Christian values in the duel and the eye gouging. But the language associated with affairs of honor was a vitally important language for most white men in the states that ultimately joined the Confederacy. It was a language spoken by owners of large plantations along the coast as well as by small farmers and landless whites in the hinterlands. There were many important connections between the language of honor and the experience of slaveownership, but these were connections that operated equally for men who owned slaves and for those who aspired to do so. The language of honor, in other words, was not confined to a single class of white males (although there were significant variations in expression among men of different classes), nor was it confined to a single region of the South.⁵

To understand the system of meanings that surrounded nose pulling in the South, it is necessary to reconstruct and connect parts of white male language that may at first appear to be unrelated. My intention is to show the rich textures of association between areas of Southern culture that have not heretofore been analyzed together. *En route to an analysis of nose pulling, this essay explains why P. T. Barnum was less popular in the South than in the North; how scientific and market activities were connected to each other but not to the world of honor; why men of honor dueled over disagreements that people outside their tradition regarded as trivial; why practical jokes had a different meaning for men of honor than for men of trade; why many anti-dueling laws required the mutilation of men who dueled; why abolitionists and proslavery apologists read the meaning of scars on the backs of slaves differently; and why the nose was more important than the genitals for Southern gentlemen.* A central organizing thread runs through and around these cultural phenomena. Each demonstrates that Southern men of honor were “superficial.” They were concerned, to a degree we would consider unusual, with the surface of things—with the world of appearances.

Honor in America (Boston, 1940); Jack K. Williams, *Dueling in the Old South: Vignettes of Social History* (College Station, Tex., 1980); Elliott J. Gorn, “‘Gouge and Bite, Pull Hair and Scratch’: The Social Significance of Fighting in the Southern Backcountry,” *AHR*, 90 (February 1985): 18–43; and Kenneth S. Greenberg, *Masters and Statesmen: The Political Culture of American Slavery* (Baltimore, 1985), 23–41.

⁵ Wyatt-Brown, in *Southern Honor*, describes the system of honor with special reference to the Southern interior and southwestern slave states. But he believes that the values of honor were not confined to a class or region within the South. Edward L. Ayers in *Vengeance and Justice: Crime and Punishment in the 19th-Century American South* (New York, 1984) also agrees that the language of honor was widely spoken all over the South. Both Ayers and Wyatt-Brown concur that one important class variation was that injured honor often led to duels among the upper classes, while the lower classes preferred fists, eye gouging, and ear biting. Wyatt-Brown disagrees with the idea of a close connection between slavery and honor. For my own discussion of the relationship between slavery and honor, see *Masters and Statesmen*. Ayers also argues that there is a strong connection between honor and slavery. See *Vengeance and Justice*, esp. 26–27.

ONE GOOD WAY TO ENTER THE LANGUAGE OF WHITE MEN OF HONOR is through an analysis of a dispute occasioned by the exhibit of the Feejee Mermaid in Charleston, South Carolina, in early 1843. This event contains no explicit mention of noses (or lies or duels), but, when we come to understand it correctly, we will recognize many oblique references. The story begins with a collaboration between Moses Kimball of Boston, owner of the mermaid, and P. T. Barnum, who arranged for its exhibition. Barnum, that quintessential "Connecticut Yankee," that great master of deceit, showmanship, and "humbug" in nineteenth-century America, undoubtedly was aware from the very beginning that the mermaid was a fake—the upper torso of a dead monkey skillfully joined to the lower body of a fish. Still, he hired a manager to take it on tour and arranged for an elaborate publicity campaign to herald its arrival in Philadelphia and New York. The exhibit met with such an enthusiastic reception in these Northern cities that Barnum decided on a Southern tour that began in Charleston, South Carolina, during the early months of 1843.⁶

The mermaid stirred more than wonder upon its arrival in the South. It became the object of a controversy that threatened to break into violence. The disturbance began in the newspapers of Charleston just after the mermaid arrived. Richard Yeadon, a local lawyer and one of the three editors of the *Charleston Courier*, wrote an unsigned review of the exhibit, venturing his opinion that the mermaid was probably a natural object and that he could detect no seam to indicate it was an artificial combination of ape and fish. "[W]e were permitted to handle and examine it as closely as could be effected by touch and sight," he wrote, "and . . . if there be any deception, it is beyond the discovery of both those senses."⁷ But, at almost the same time, a very different article appeared in the rival *Charleston Mercury*. Writing under the name of "No Humbug," the respected naturalist and Lutheran minister John Bachman declared the mermaid to be a fraud, "a fishes tail attached to the head and shoulders of a Baboon," "a clumsy affair," "a smoke dried affair" created by "our Yankee neighbors."⁸ The debate initiated by these notes led to the production of over two dozen published letters during the next few months—some of considerable length, occasionally occupying over one-quarter of all the article space in the major South Carolina newspapers.

Among the dispute's many interesting features was Bachman's contention that the editors of the *Courier* had failed to treat him with respect. He had originally come to them with his letter denouncing the exhibit as a fraud. They refused publication because they found the language "too severe." He then brought it to the *Mercury*, where they promptly printed it—only to discover that the *Courier* had not only rejected his letter but also published an editorial review in support of the mermaid.⁹

At the same time, Bachman also raised the question of who was competent to pass judgment on the mermaid. Joined by other South Carolina scientists, he wondered why the lawyer and editor Yeadon felt qualified to make statements about matters scientific. Bachman had seen a seam at the juncture of fish and monkey, and, if Yeadon did not detect one, it was only because he was "untrained in such observations."¹⁰ Yeadon conceded that he was no scientist and emphasized that he did not make an unequivocal claim about the authenticity of the mermaid—only

⁶ Neil Harris, *Humbug: The Art of P. T. Barnum* (Chicago, 1973), 62–67.

⁷ *Charleston Courier*, January 21, 1843.

⁸ *Charleston Mercury*, January 20, 1843.

⁹ *Charleston Mercury*, February 1, 1843.

¹⁰ *Charleston Mercury*, February 1, 1843.



THE POUTER MERMAID first surfaced in Boston in 1824. The eventual owner, Moses Kimball of the Boston Museum, lent it to P. T. Barnum, who made it famous throughout the eastern seaboard in the early 1840s. Later, it returned to the Boston Museum, where it had star billing for several decades among the "half a million curiosities." Photo by Hillel Burgen, courtesy of the Peabody Museum, Harvard University, Cambridge, Massachusetts.

that, when he took it from the case, he could neither see nor feel a seam. He cared "not a whit, not a stiver, whether the Mermaid is real or not."¹¹ He only demanded that his observation be treated with respect. "Mr. No Humbug," Yeadon claimed, had unfairly attacked him: "he has assaulted us whip in hand, but we mean to take it from him and lay it, and that smartly, on his own shoulders."¹²

Another theme of the dispute involved the use of names and pseudonyms. The original articles were either unsigned editorials, or signed with noms de plume such as "No Humbug" and "The Man Who Exhibits the Mermaid." But, after February 6, the real names began to appear in print. This was an important transition. Yeadon apparently had long felt that, as editor of the *Courier*, he could not maintain anonymity as easily as could Bachman. An inequality had developed. He objected to his opponent's practice of addressing his letters to "the editor of the *Courier*." There were three editors, and it was offensive, he felt, to single out one by not using the plural form of address. The singular form of address had begun to move the dispute to a more personal and dangerous level. Moreover, Yeadon believed that Bachman had mentioned his name to other people, and he objected to the fact that "it [his name] was a common subject of conversation out of doors promiscuously."¹³ In fact, the transition from anonymity to names was so delicate that it required the intervention of "a mutual friend" to prevent the dispute from ending in an exchange of pistol shots.¹⁴ The mutual friend negotiated an agreement that allowed for the simultaneous public use of real names by each party.

But what is most startling about the debate over the Feejee Mermaid is the lack of interest in the mermaid itself. While in some other contexts, the scientists might have had a desire to discover whether or not this was a natural mermaid, neither they nor Yeadon devoted much of their newspaper discussion to that topic. More important were such matters as whether Bachman had been handled with dignity by the *Courier*, whether Yeadon's observation that he could see no seam had been treated with proper respect by the scientists, and whether the parties had properly or improperly used and treated pseudonyms supplied by the writers. It was not a debate about whether the Feejee Mermaid was a real mermaid or a hoax. Yeadon himself had said so unequivocally. He "cared not a whit, not a stiver, whether the Mermaid is real or not." Although the mermaid had left town by the end of January, the dispute continued to rage until late March. The central concern of these men was to have their words, names, and pseudonyms treated with respect. Honor was at stake. Penetration into the secrets of nature was of little interest.

IF THE FORM AND CONTENT OF THE DISPUTE over the Feejee Mermaid had been an isolated event in antebellum Southern history, it could be dismissed as trivial. But more is involved here. The lack of interest in the Feejee Mermaid and in other secrets of nature appears everywhere in the antebellum South. For example, the incident may help us understand why scientists and other intellectuals in this culture felt unappreciated. Historians have long been aware of the complaints of

¹¹ *Charleston Courier*, February 6, 1843.

¹² *Charleston Courier*, February 1, 1843.

¹³ *Charleston Courier*, February 6, 1843.

¹⁴ *Charleston Courier*, February 6, 1843.

neglect by the intellectuals of the South.¹⁵ Some have attributed the problem to the absence of cities and the consequent lack of a reading public concentrated in urban areas. Others have suggested that intellectuals play a marginal role in most societies. While these explanations offer some insights into the problems of Southern thinkers, the Feejee Mermaid episode points in another direction.

If a central concern of nineteenth-century scientific and intellectual activity was to penetrate into the secrets of nature, to move from the level of superficial appearance to a deeper and hidden reality, then it may have involved a sensibility alien to antebellum Southern white male culture. Many cultures concerned with honor highly value appearance. Their members project themselves through how they look and what they say.¹⁶ They are treated honorably when their projection is respected and accepted as true. The central issue of concern to men in such a culture is not the nature of some underlying reality but the acceptance of their projections. (They "care not a whit" about the reality of the mermaid.) The men who achieve the most honorable positions in such a culture are statesmen—men whose vision of themselves and their world is confirmed by popular acclamation.¹⁷ The intellectual, who conceives of his activity as exposing hidden levels of reality, is engaged in work of peripheral interest.¹⁸

The Feejee Mermaid episode also echoed the world of dueling—the ritual that embodied many values at the core of antebellum Southern white male culture.¹⁹ First, the form of the Feejee Mermaid dispute was remarkably similar to the form of typical nineteenth-century dueling encounters. In both cases, the conflict originated from insulting words or actions. Then followed a carefully worded exchange of letters in which each party tried to describe how he had been injured—how he had not been treated with the kind of courtesy due to him as a social equal. There was the intervention of another party whose job it was to bring about a reconciliation through a carefully constructed understanding in which each party explained his language and acknowledged his equality with the other. Even the publication of the details of the Feejee Mermaid dispute paralleled a practice typical of dueling encounters. A duel was a theatrical display for public consumption, and parties expected descriptions of the events to be widely circulated. The Feejee Mermaid episode did not lead to an exchange of pistol shots, but neither did the vast majority of conflicts that were part of the world of dueling.²⁰

Second, participants in dueling encounters and in the Feejee Mermaid episode expressed similar conceptions of what constituted an insult. At the heart of both encounters was the accusation of lying. Consider the importance of lying as an insult in a typical dueling dispute. While white men of the South came into conflict with each other on many issues, they did not always duel. Only certain kinds of

¹⁵ See especially the discussion of this issue in Drew Gilpin Faust, *A Sacred Circle: The Dilemma of the Intellectual in the Old South, 1840–1860* (Baltimore, Md., 1977).

¹⁶ For discussions of honor that emphasize similar values in other societies, see J. G. Peristiany, ed., *Honour and Shame: The Values of Mediterranean Society* (London, 1965); Christopher Boehm, *Blood Revenge: The Enactment and Management of Conflict in Montenegro and Other Tribal Societies* (Philadelphia, 1984); Stanley Brandes, *Metaphors of Masculinity: Sex and Status in Andalusian Folklore* (Philadelphia, 1980); Lila Abu-Lughod, *Veiled Sentiments: Honor and Poetry in a Bedouin Society* (Berkeley, Calif., 1986); and Orlando Patterson, *Slavery and Social Death: A Comparative Study* (Cambridge, Mass., 1982).

¹⁷ On the significance of statesmanship in Southern culture, see Greenberg, *Masters and Statesmen*, 3–22.

¹⁸ Southern intellectuals could achieve honor only by entering the world of the statesman. That may explain their attraction to the proslavery argument. See Faust, *Sacred Circle*, 112–31.

¹⁹ Greenberg, *Masters and Statesmen*; Williams, *Dueling in the Old South*, 1–12.

²⁰ On the form of the ideal duel, see John Lyde Wilson, *The Code of Honor or Rules for the Government of Principals and Seconds in Duelling* (Charleston, S.C., 1838).

insulting language and behavior led to duels. The central insult that could turn a disagreement into a duel involved a direct or indirect attack on someone's word—the accusation that a man was a liar. To “give someone the lie,” as it was called, had always been an insult of great consequence among men of honor. As one early seventeenth-century English writer noted, “It is reputed so great a shame to be accounted a lyer [*sic*], that any other injury is canceled by giving the lie, and he that receiveth it standeth so charged in his honor and reputation, that he cannot disburden himself of that imputation, but by the striking of him that hath so given it, or by chalenging [*sic*] him the combat.”²¹

It is easy to underestimate the importance of “giving the lie” as a cause of Southern duels unless we have a broad understanding of what Southern white men had in mind by “lying.” To “give the lie” to someone was to announce that their appearance differed from their true nature—to proclaim as false their projection of themselves. Thus the charge of being a coward or a poltroon was another form of the charge of lying—the accuser unmasked the false face of the accused. The goal of this unmasking was not to discover the real character underneath but to expose and shame an opponent. It was to identify an image as falsely projected and to show contempt for it. This was not a charge made by a scientist in search of hidden truths but by a gentleman intending to dishonor someone. For instance, the accusation of the lie lay behind the charge of one man as he pointed out his enemy to an audience. “Citizens,” he announced, “this is not the profile of a man; it is the profile of a dog!” Similarly, when a man woke up one day, as did Charles A. Luzenberg in antebellum New Orleans, opened his morning newspaper, and read a note accusing him of being a “puppy” who has “long humbugged the community with false ideas of his courage,” he understood that someone was trying to unmask him, was accusing him of lying.²²

The Feejee mermaid episode also involved the charge of lying. Yeadon worried that his words in support of the mermaid had been discounted as false. He had run his hands over the surface of the creature and could detect no seam. His projection of the truth and thereby of himself had become connected to the appearance of the mermaid. Bachman, after having his own words rejected for publication in Yeadon's paper, exposed the humbuggery of the mermaid as a “clumsy affair” and thus came close to accusing Yeadon of lying. Moreover, the dispute over the use of pseudonyms was actually another, even more complicated, conversation about the relationship between asserted appearance and reality. The discussion touched on the dangerous issue of the connection between a man's real character and the character projected by a pseudonym. As the dispute moved from pseudonyms to names, Yeadon and Bachman each had to avoid the public perception that they had been unmasked and thereby shamed by their opponent. To avoid a confrontation that could end in a duel, each had to move from pseudonym to real name simultaneously—each had to unmask himself and have his new projection immediately accepted by his opponent.

What we see in both the duel and the Feejee Mermaid episode is evidence of a world that places a high value on appearances as asserted and projected through the words of honorable gentlemen. Many Northern men, the type who shared the values of a mermaid exhibitor like P. T. Barnum, would never fully understand this

²¹ Quoted in Robert Baldick, *The Duel: A History of Dueling* (London, 1965), 33. Baldick also notes that Touchstone in William Shakespeare's *As You Like It* lists the various causes of quarrels among gentlemen. He notes that you can avoid a fight for all causes except “the lie direct.”

²² Quotations can be found in Williams, *Dueling in the Old South*, 23.

world. This is not to suggest that men like Barnum failed to appreciate the value of self-promotion and the manipulation of appearances. Barnum, after all, was the great showman of the nineteenth century. He was the virtual founder of modern American print advertising. How could the man who transformed Joice Heth, an elderly and nearly blind black woman, into "the most astonishing and interesting curiosity in the world," the 161-year-old former nurse of George Washington—how could the man who turned Tom Thumb from very short person into world celebrity—how could the man who changed a large mammal into Jumbo the Elephant and then used him to advertise thread and baking powder—not value the world of appearances?²³ Yet it was precisely because he could manipulate such a world and simultaneously make money from it that Barnum could not see it as worthy of serious respect or disrespect. Words were his instruments in pursuit of gain and not at all linked to honor or dishonor. Perhaps nothing so clearly sums up Barnum's attitude toward the surface of images and language as his description of his first business experience in a Connecticut country store: "The customers cheated us in their fabrics, we cheated the customers with our goods. Each party expected to be cheated, if it was possible. Our eyes, and not our ears, had to be our masters. We must believe little that we saw, and less that we heard."²⁴ In short, the world of trade and bargaining was a world full of liars. People constantly unmasked each other. But at stake were profit and loss rather than honor and dishonor. When Bachman called his mermaid a fake, Barnum did not reach for his gun but instead thought of ways to turn the charge to his profit.²⁵

To recognize the connection between Barnum's experience of trade and his attitude toward appearance is not to make the larger reductive point that economic experience creates meaning. Economic experience does not exist prior to and outside a world of meaning, and therefore it cannot constitute such a world in any simple sense. Antebellum Americans did not first experience economics and then create meaning. Their experience of economic activity came to them already filtered through a system of meaning. Bachman and Barnum did not differ from each other simply because one traded and the other did not. In fact, it seems clear that many Southern men who spoke the language of honor were deeply involved in trade and bargaining. But men who spoke the language of honor experienced trading and bargaining differently than did Barnum. To reconstruct their language is to reconstruct their different experience. Of course, to recognize that economic experience is filtered through language is not to make the equally reductive point that language creates experience; it is only to understand that language and experience are not two separate categories.²⁶

Just as it is possible to connect Barnum's experience of trade to his ideas about

²³ For a full description of Barnum's career, see Harris, *Humbug*.

²⁴ Quoted in Harris, *Humbug*, 12–13.

²⁵ In fact, Barnum himself always opposed the duel; see P. T. Barnum, *Struggles and Triumphs or, Forty Years' Recollections of P. T. Barnum*, Carl Bode, ed. (1855; New York, 1981), 86, 138. On Barnum's lack of success in the South, see Harris, *Humbug*, 66–67. For a larger discussion of the way Barnum's attitude toward lies ultimately became widely shared in American middle-class culture, see Karen Halttunen, *Confidence Men and Painted Women: A Study of Middle-Class Culture in America, 1830–1870* (New Haven, Conn., 1982).

²⁶ For a critique of the approach that assumes language is dependent on "objective," or extralinguistic, phenomena, see Toews, "Intellectual History after the Linguistic Turn," 879–907; LaCapra and Kaplan, *Modern European Intellectual History*; and Scott, *Gender and the Politics of History*. For the idea that Southerners could engage in trade without fully adopting the bourgeois values associated with men of trade, see Eugene D. Genovese, *Roll, Jordan, Roll: The World the Slaves Made* (New York, 1974); and Elizabeth Fox-Genovese, *Within the Plantation Household: Black and White Women of the Old South* (Chapel Hill, N.C., 1988).

appearance, we can relate the white Southern experience of slavery to the white Southern attitude toward the surface of the world. Masters and potential masters distinguished themselves from slaves in many ways, but one of the most important distinctions involved the issue of lying. The words of the master had to be accorded respect and accepted as true simply because they were the words of a man of honor. The words of the slave could never become objects of honor. Whites assumed that slaves lied all the time—and that their lies were intimately connected to their position as slaves. Masters articulated these beliefs when they argued that it was absurd for slaves to engage in duels or that black testimony could never be used in legal cases involving whites. Moreover, for many masters, the lie was at the heart of their problems with slave labor. Instead of the open confrontation expected of men of honor, slaves seemed to resist their masters by stealth and deceit. They stole food, ran away, burned buildings, broke tools, feigned illness or laziness, and sometimes even poisoned their master's or mistress's food. At one level, masters expressed repeated exasperation over the deceit of their slaves. But, in another sense, masters welcomed the chance to catch their slaves in lies. Their own honor and the respect accorded their words could then stand in favorable contrast to the dishonored condition of their slaves.²⁷

THE CLASH BETWEEN THE LIE as expressed in the world of the duel and the world of P. T. Barnum recurs in other places in nineteenth-century American culture. For example, men of honor and P. T. Barnum reacted quite differently to lies as expressed in practical jokes—a form of play that Barnum loved. In a way, his entire career was based on a series of practical jokes—lying to the public in order to make money. But Barnum also enjoyed the practical joke outside the world of show business. He especially enjoyed tricking a man who was trying to trick him. In his autobiography, Barnum proudly told the story of “the whole shirt” trick. A noted joker named Darrow used to frequent the barroom of a Bridgeport hotel and prey on unsuspecting guests. A favorite trick of Darrow's was to taunt a man with the declaration that he was probably wearing tattered clothes and did not have a “whole shirt” on his back. Darrow would persuade the man to bet that he did have a whole shirt on his back and then triumphantly point out that all men only wore half their shirts on the backs. One day, Barnum deliberately allowed himself to be drawn into the bet, then produced a “whole shirt” that he had carefully folded onto his back beneath his vest. Darrow was furious, but he paid what he owed, and the entire barroom had a good laugh and a round of drinks. As in tricks played in the marketplaces frequented by men of trade, Barnum did not feel his life to be in danger from this joke. Barnum had tricked Darrow this time. Darrow would trick

²⁷ The lie was so central to the slave experience in the eyes of both masters and slaves that virtually every Southern primary and secondary source could be used in illustration. For specific examples, see Gilbert Osofsky, ed., *Puttin' on Ole Massa: The Slave Narratives of Henry Bibb, William Wells Brown, and Solomon Northup* (New York, 1969); and Deborah Gray White, *Ar'n't I a Woman?: Female Slaves in the Plantation South* (New York, 1985). For the connections between honor and slavery in many slave societies, see Patterson, *Slavery and Social Death*.

White women seemed to resemble slaves in their lack of power—their lack of the ability to directly confront and ultimately duel with or pull the noses of people who insulted them. But, overall, white men probably saw white women as somewhat less deceitful than slaves. See Anne Firor Scott, *The Southern Lady: From Pedestal to Politics, 1830–1930* (Chicago, 1970), 3–21; Catherine Clinton, *The Plantation Mistress: Woman's World in the Old South* (New York, 1982); and Fox-Genovese, *Within the Plantation Household*.

Barnum and others the next time. That was life in the world of the market.²⁸

But a different set of meanings attended practical jokes among men of honor. Consider the experience of J. Marion Sims, the well-known gynecologist, during his years in South Carolina. Sims describes one incident parallel to Barnum's joke except in its markedly different consequences. While he was at South Carolina College, Sims roomed and boarded with a group of other young Carolina gentlemen, including James Aiken and Boykin Witherspoon. One day, as these young men were about to sit down to dinner, Aiken playfully pulled Sims's chair away just as Sims was about to be seated. Sims, however, seeing the motion out of the corner of his eye, did not sit down but reached over and pulled Witherspoon's chair away from him. Witherspoon fell on the floor. Unlike Barnum's experience with tricks, however, no one thought this was funny. This was a moment of danger, not of laughter. One man had manipulated the world of appearances in order to humiliate another man. Sims immediately understood the seriousness of what had transpired. As he later described it, "I apologized in the humblest manner that I possibly could. I assured him that I did not intend to throw him down, that I regretted it then, and that I was not ashamed to say that I was heartily sorry and should regret it always." Witherspoon said that he was not satisfied. After dinner, he confronted Sims again. Sims repeated the abject apology, yet Witherspoon was still not satisfied. At this point, Sims believed he had humbled himself as much as was possible for a gentleman. "Now Sir, help yourself," Sims finally blurted, meaning that he was willing to give Witherspoon satisfaction in a duel. Sims would apologize no more for fear that his friends, witnesses to the entire series of events, might think that he was humbling himself in order to avoid risking his life in a duel.²⁹ Everyone understood that this kind of joke in South Carolina did not end in laughter and a round of drinks. It was likely to end in death.

We can also see conflicting conceptions of the lie between men inside and outside the culture of honor in the failure of many critics of the duel—men frequently outside Southern culture—to make sense of the world of duelists. Benjamin Franklin, for example, wrote to a friend in 1784 that he could not understand the logic of the duel: "A man says something which another tells him is a lie. They fight, but whichever is killed the point in dispute remains unsettled." Franklin continued with an anecdote to illustrate the point. A famous French duelist, St. Froix, was sitting at a cafe and turned to a stranger, asking him to move away because he smelled. "This is an affront," exclaimed the stranger, "you must fight me." "I will fight you if you insist on it," St. Froix replied, "but I do not see how that will mend the matter, for if you kill me I shall smell too, and if I kill you, you will smell, if possible, worse than you do at present."³⁰ Whether or not the anecdote is true, men of honor would not have gleaned from it the point made by Franklin. For him, the duel seemed a pointless activity because it could not determine whether a man had really lied; it had no practical result. If Franklin had been involved in the Feejee Mermaid dispute, he probably would have used the same logic to push for an internal examination of the mermaid rather than to linger over seemingly trivial insults. But men of honor care "not a whit" about real mermaids or real lies or real smells. What is central to their disputes is that someone has given expression to

²⁸ Barnum, *Struggles and Triumphs*, 77–78.

²⁹ Luckily, in this case, Witherspoon never issued the challenge. The record does not reveal the reasons. See J. Marion Sims, *The Story of My Life* (New York, 1884), 100–02.

³⁰ Benjamin Franklin, quoted in William Oliver Stevens, *Pistols at Ten Paces: The Story of the Code of Honor in America* (Boston, 1940), 6.

insult. When the man of honor is told that he smells, he does not take a bath—he draws his pistol. In other words, a man of honor does not care if he stinks, but he does care that someone has accused him of stinking. Timothy Dwight, in his famous anti-dueling sermon delivered at Yale University after the Aaron Burr–Alexander Hamilton affair, misunderstood the duel in precisely the same way as Franklin. He could not figure out why someone would duel if he had been called a liar. “If the charge is just,” he pointed out, “he is a liar still.” For Dwight, the point was simple: “Truth and falsehood must, if evinced at all, be evinced by evidence; not by fighting.” Neither could understand the nature of an insult to a man of honor.³¹

Some critics of the duel did understand the causes of these encounters, and they wrote laws designed to eliminate duels, not that any anti-dueling law ever worked to repress the institution. Most people refused to treat men of honor in the ways demanded by eighteenth-century anti-dueling laws. But many of these laws at least show an understanding of the nature of the duel. For example, one law provided that the dead duelist should be “buried without a coffin, with a stake drove through the body.” The survivor should be executed and similarly buried. A later revision offered an alternative to burial with a stake. The body could be delivered to “any surgeon or surgeons to be dissected and anatomized.”³² Other laws ordered gibbeting—hanging the dead bodies in a cage of chains to rot in public. But, in the United States by the nineteenth century, the most common anti-dueling penalty had become a disqualification from holding office. This may seem a rather strange development, implying an extraordinary discontinuity. Actually, there is similarity between the stake through the heart, the gibbet, and the disqualification from office: all are ways of dishonoring a person in a culture of honor. Moreover, all involve the same technique: they are ways of preventing a man from creating an image of himself that might become an object of public honor. To withhold office from the statesman is to prevent the public from confirming his vision of himself. To mutilate his dead body is to turn his physical projection into an ugly object worthy of scorn and shame.

In fact, the anti-dueling laws that struck at the body of the duelist show that legislators understood the nature of a man of honor. To gouge out an eye or otherwise mutilate the face of an enemy was the most common object of men involved in fist fights.³³ With the possible exception of battle wounds, the mutilation itself was dishonoring, no matter how it was actually acquired. In a sense, all mutilations were equal, because men read the character of other men through the external features of their face and body.³⁴ This attitude toward appearance was also given expression in the many letters of reference or descriptions of great men that mentioned the look of their noble features. One can also see it in the way white Southerners used skin color as a way of determining the character of a man.

The same attitude toward the external appears in the masters’ interpretation of scars on the backs of their slaves. For white Southerners, the mark of the whip on

³¹ Timothy Dwight, *The Folly, Guilt, and Mischiefs of Duelling: A Sermon Preached in the College Chapel at New Haven, On the Sabbath Preceding the Annual Commencement, September, 1804* (Hartford, Conn., 1805), 9.

³² Evarts B. Greene, “The Code of Honor in Colonial and Revolutionary Times, with Special Reference to New England,” *Publications of the Colonial Society of Massachusetts*, 26 (Boston, 1927), 375, 387.

³³ Gorn, “Gouge and Bite, Pull Hair and Scratch”; Ayers, *Vengeance and Justice*, 9–10.

³⁴ Ayers, *Vengeance and Justice*, 43.

the back of a slave was a sign of the slave's bad character and "vicious temper."³⁵ When the abolitionists disagreed, interpreting the scars on the slave as a sign of the bad character of the master or as an expression of the evils of enslavement, they were clashing with white Southerners in a fundamental way. For this dispute involved not simply a difference of interpretation over the sign of the scar but also a different conception of the nature of reading signs. The abolitionists read for meaning beneath and beyond the surface. They found it important to imagine the scene behind the scar—and they re-created it endless times in word and woodcut. But men of honor did not linger over the scene that gave rise to the scar; it was irrelevant. The scar, in a sense, spoke for itself—or rather spoke about the man to whom it was attached—regardless of the process or the larger set of relations that brought it into existence. Men of honor "care not a whit" if the mermaid is real.

ALL THIS BRINGS US TO THE NOSE. For Southern men of honor, the nose was the part of the face that preceded a man as he moved in the world. It was the most prominent physical projection of a man's character and always exposed before the eyes of others. Little wonder that men of honor should regard the nose as the most important part of their bodies. As one antebellum Southern writer described it in a humorous, but also deadly serious, article on noses, "No organ of the body is so characteristic as the nose. A man may lose an eye or an ear without altering his features essentially. Not so with the nose." He went on to describe a man with "a most extravagantly protuberant nose"—a nose that "moved to and fro like a pendulum"—who had decided to have it trimmed by a doctor. "I saw him afterwards," he wrote, "and did not recognize him. I do not recognize him now, nor do I intend to. His individuality, his whole identity is lost. . . The features do not fit; they become incongruous; he is himself no more; for, in truth, the individuality of a man is centred in his nose. Hence it is that nature, to indicate its great importance, has granted us but one nose, while all other organs are supplied in pairs."³⁶ That this writer ignored internal organs is another illustration of the external focus of a man of honor. Clearly, he was comparing the nose to the eyes and the ears. The liver, the heart, the penis, and the stomach were not even possible candidates of great significance. A man's character was expressed in what could be publicly displayed, not in what was hidden under clothes or skin. And, of course, the man of honor demanded respect for this display. If P. T. Barnum, a man outside the tradition of honor, had a nose that drew laughs, he might have charged admission; Cyrano de Bergerac, on the other hand, fought duels with those who mocked his protuberance.

A man of honor valued public acclamation for the parts of a man that are visible to the public, including the visible surface of his body, his words, and his version of the truth. Thus it makes sense that one of the greatest insults for a man of honor was to have his nose pulled or tweaked. Actually, nose pulling was just another, more aggressive, form of accusing a man of lying. It was the ultimate act of contempt toward the most public part of a man's face—an extreme expression of disdain for a man's projected mask. We can begin to understand the meaning of

³⁵ See, for example, John W. Blassingame, ed., *Slave Testimony: Two Centuries of Letters, Speeches, Interviews, and Autobiographies* (Baton Rouge, La., 1977), 632.

³⁶ *Russell's Magazine*, 3 (September 1858): 538.

nose pulling for men of honor through a look at one well-documented incident of the 1830s: when Thomas Walker Gilmer pulled the nose of William C. Rives.

The pulling of Rives's nose had its origins in a friendship and political alliance gone sour. Rives and Gilmer had long been close associates in Virginia politics as well as neighbors with frequent social relations. During the 1820s, they worked together as opponents of the John Quincy Adams administration, and Rives had hopes that Gilmer might one day succeed him in Congress. Rives helped to advance Gilmer, and Gilmer did the same for Rives. With the enthusiastic support of Gilmer in the state legislature, Rives returned home in 1832 after serving as ambassador to France and was immediately elected U.S. senator. When other legislators raised doubts that Rives might not be sound on the tariff issue—that he might not take the position that it was unconstitutional and that he might not support South Carolina's attempt to nullify the law—Gilmer assured his skeptical colleagues that they could rely on his friend.³⁷

But relations began to deteriorate when it became clear that Rives would not live up to the expectations created by Gilmer. Contrary to what Gilmer had indicated, Rives did not view the tariff law as unconstitutional. Moreover, he voted for the much-hated "Force Bill"—Andrew Jackson's heavy-handed attempt to coerce the South Carolina nullifiers into submission. This "betrayal" became intertwined with more specific grievances detailed in letters exchanged between the two men. Gilmer had several causes of complaint. First, Gilmer had given his word to other men that Rives could be trusted on the tariff issue. Rives's vote on the "Force Bill" meant that these men had come to regard Gilmer as a liar. "I have been taunted more than once," Gilmer wrote Rives, "of having abused the confidence of those who listened to my appeal on your behalf." In fact, shortly after he pulled Rives's nose, Gilmer tried to show the public that he had legitimate grievances against Rives by publishing all relevant correspondence. He included letters solicited from his colleagues in the legislature attesting to the fact that they "distrusted" him because his words about Rives had been shown to be false. In short, Gilmer stood in the same relation to Rives as Richard Yeadon did in relation to the Feejee Mermaid. Both men became identified with the object for which they spoke.³⁸

Gilmer had a second major grievance. Rives, in turn, had begun to denounce Gilmer as a liar. Rives accused Gilmer of appearing to be a friend on the surface while actually working against him in devious and hidden ways. Rives believed (erroneously, it turns out) that Gilmer was the author of an anonymous and very critical article sent to the *Richmond Enquirer*; that, writing under the name "Buckskin" in the *Charlottesville Advocate*, Gilmer had viciously attacked him in print; and that Gilmer was behind a Virginia legislative attempt to instruct Rives on how to vote on the "Force Bill" when it came before the Senate. Gilmer defended himself by acknowledging that he and Rives disagreed over the tariff issue but maintaining that he never tried to give any other impression. In other words, while Rives said that Gilmer's appearance differed from his reality, Gilmer argued that the two were in complete conformity. Gilmer claimed he had always been open and

³⁷ Drew R. McCoy first brought the Rives-Gilmer dispute to my attention and then shared a draft of his own remarks on the affair subsequently published in *The Last of the Fathers: James Madison and the Republican Legacy* (Cambridge, 1989).

³⁸ The analogy extends to the way both men "cared not a whit" about the real nature of the object itself. Gilmer's honor would not have been offended if Rives had simply voted for the "Force Bill." In fact, in his letters to Rives, Gilmer makes it clear that their political differences alone would not have destroyed their friendship. The real grievance was that, because Gilmer had spoken for Rives, Rives's betrayal had led men to distrust Gilmer's word.

honest about their differences and that these differences existed within the context of their friendship.³⁹

After months of festering distrust and hatred, the Rives-Gilmer dispute reached its climax at the courthouse in Charlottesville, Virginia, in early July 1833. The two men's deteriorating relations up to that point could be followed in the salutations Rives used in his letters to Gilmer: first "Dear Gilmer," then "Dear Sir," then "Sir." By the time Rives began to call Gilmer "Sir," it must have looked like they were close to some violent outbreak—perhaps even on the edge of fighting a duel. In Virginia society of the 1830s, a duel was a logical result of this kind of dispute. By shooting at each other, men accused of lying could show the world that they would rather die or kill than allow the charge to stand.

But the Rives-Gilmer dispute did not reach the stage of a duel. Instead, Gilmer approached Rives on the terrace of the tavern next to the courthouse in Charlottesville. They began to go over their charges and countercharges, deciding first to move into the public room of the tavern and then into a more private back room. They could agree on nothing. Words between the two men became increasingly heated. Gilmer called Rives a "hypocrite," and Rives retorted that Gilmer was a "scoundrel." There are two versions of what happened next. As Gilmer described it, "I then applied my right hand gently to his nose. He instantly disengaged himself from me, either by drawing back or pushing me from him, and having a horsewhip in his hand, struck me several times with the butt end of it. While I parried these blows with my right arm, I attempted to catch him by the collar of his coat with my left hand, and in this effort the fore-finger of my left hand got into his mouth and was severely bitten. In the attempt to extricate it, my right thumb was painfully injured. While my finger was thus in his mouth I struck him two blows in the face with my right hand." Gilmer then pulled the whip from Rives and used the "smaller end" to inflict "several stripes with it on his legs and shoulders and (I think) one on his forehead." At that moment, a crowd rushed in and separated the two struggling men.

The account of the fight offered by Rives and his supporters agreed with this description in all but one respect. The pro-Rives account emphasized that Rives was attacked completely by surprise and that he was seated while Gilmer stood and assaulted him. Gilmer denied this version of the attack. He preferred to portray himself and Rives as equals in combat. He also stressed that it was not his intent to draw blood or to hurt Rives in any way. He meant to apply his right hand "gently" to Rives's nose. The rest of the scuffle was in self-defense. As he later stated, "My purpose throughout has been to vindicate myself—not to injure Mr. Rives."

Several features of this nose-pulling incident are worthy of special emphasis here. Just as in the Feejee Mermaid episode, the dispute that led up to the attack was essentially about the proper treatment to be accorded the word of a gentleman. At the heart of it were accusations of lying. It is easy to miss this point. One might be tempted to say that the conflict was really a disagreement about the tariff issue or nullification. In one sense, this would be correct—because it is impossible to imagine this series of events without the precipitating political dispute that gave rise to it. Similarly, it is impossible to imagine the Feejee Mermaid dispute without a mermaid. But it is also clear from the angry letters in both cases that, at least in this context, the men involved did not focus on the substance of the matter that gave

³⁹ The relevant correspondence on which this and subsequent descriptions of the Rives-Gilmer dispute are based can be found in the *Richmond Enquirer*, July 26, 1833.

rise to the dispute. In the same way that Yeadon “cared not a whit” about the mermaid, Gilmer and Rives never discussed the substance of the tariff issue or nullification in their correspondence that led to the nose pulling. Many men disagreed over the tariff and nullification. But those disagreements did not lead to nose pullings or duels unless a man’s character came under attack. This point is essential if we are ever to understand why the pulling of a nose seemed the appropriate remedy. Gilmer had been accused of lying—of putting forth a projection of himself that was false. He pulled Rives’s nose to show his contempt for the projection of his accuser. It was his way of invalidating the words of his enemy. The nose pulling was not primarily part of a conversation about the merits of the tariff or nullification. It was part of a conversation about lying.

ANOTHER REMARKABLE FEATURE OF THIS NOSE PULLING is the evidence it offers that the community seemed to regard the nose as a kind of sacred object. In the newspaper reports of this episode, virtually the only person who uses the word “nose” is Gilmer. But his use of the word is a kind of extension of his pulling of it—part of the way he humiliates Rives. Newspaper editors found ways of avoiding direct reference to the nose or labeled the act of nose pulling a “Lieutenant Randolph outrage.” The reference here, well understood by every Virginian of the time, was to another extraordinary nose-pulling incident, worth describing because it even more dramatically illustrates the symbolic nature of the nose. The “Lieutenant Randolph outrage” was an attack on perhaps the most highly venerated nose of the age—that of President Andrew Jackson.

Lieutenant Randolph was a naval officer from Fredericksburg, Virginia, who had been dismissed in disgrace under direct orders from Jackson. A full account of the circumstances that led to his attack on Jackson could occupy many, many pages. The entire affair had causes and consequences extending over nearly ten years and involving some of the most prominent political leaders and events of the Jacksonian era. In 1828, Randolph had been appointed purser aboard the USS *Constitution* after the sudden death of the former purser, John B. Timberlake. An auditor’s report and subsequent investigation had found that Randolph’s accounts did not balance, that he was a debtor to the government, but that there was no evidence of intentional wrongdoing. However, on the strength of this investigation, Jackson dismissed Randolph from the navy, noting that “the facts which appear in this case, and the *conduct of Lieut. Randolph throughout the investigation*, prove him to be unworthy the Naval Service of this Republic, and *an unfit associate for those sons of chivalry, integrity, and honor, who adorn our Navy.*”⁴⁰

Randolph felt himself unjustly treated by Jackson on several counts. He had interpreted the investigative report as a vindication of his conduct since it found no intentional wrongdoing. Moreover, he believed that the man who actually embezzled the money was his predecessor. He also believed that Timberlake had funneled some of that money to John H. Eaton—a man who had become secretary of war by the time the controversy became a major public issue. Randolph probably suspected an even greater conspiracy but was too discreet ever to describe it for the public record. The famous socialite Peggy O’Neale had been Timberlake’s wife, and upon his death she married John H. Eaton with the kind of unseemly haste that excited the Washington rumor mill. For complex reasons, probably rooted in

⁴⁰ Quoted in *National Intelligencer*, May 13, 1833, italics in original.

associations with his own wife, Jackson felt some need to protect Peggy O'Neale. His dismissal of Randolph may have been part of a defense of the good name of O'Neale's first husband and thereby of her good name.

At any rate, by 1833, Randolph was a disgraced ex-naval officer who perceived Jackson's unjust actions as the cause of his misery. Since no sitting U.S. president would possibly condescend to fight a duel with a dismissed naval officer, Randolph saw only one route open for vindication—an attack on Jackson's nose. What better occasion for the assault than President Jackson's trip on May 6 to Randolph's home town of Fredericksburg in order to lay the cornerstone at a monument to the memory of George Washington's mother? One can imagine Randolph's sense of outrage—to have the very man who had humiliated him come to Randolph's own town to be honored. Jackson made the trip by boat, and, at a brief stopover in Alexandria, Randolph boarded along with many others wishing to offer their greetings to the president. After most of the well-wishers had left, Randolph made his way into a cabin where Jackson was seated, surrounded by several members of his party.

There are two versions of what happened in the cabin. The version used in virtually all newspaper accounts of the assault was the one most favorable to the dignity of the president. According to this story, Randolph approached the aged Jackson with "timidity" and "humility." His hands trembled as he tried to draw off his glove, and the president, supposing that Randolph wished to shake his hand, said that it was not necessary to remove the glove. The pro-Jackson accounts use several different expressions to describe what happened next. Most say that Randolph "thrust one hand violently into the President's face" or that Randolph "struck him in the face."⁴¹ Hardly any account mentions Jackson's nose either as the object of the intended or of the actual attack.

Descriptions of Jackson's reaction to the assault clearly indicate that Jackson and all the bystanders understood that the president's honor was under attack, not just his body. Jackson immediately "thrust the dastardly assailant from him" and stood up. As men rushed in to restrain Randolph, the sixty-six-year-old Jackson grabbed his cane, demanding that everyone move away and leave him free to wreak vengeance on his attacker. "Let no man stand between me and the villain," an eyewitness reported Jackson to have said.⁴² Another witness, perhaps with a bit more embellishment, claimed that the president said, "Let no man interfere between me and this personal insult; I am an old man, but fully capable of defending myself against, and punishing a dozen cowardly assassins."⁴³ Jackson himself later wrote Martin Van Buren that if he had been prepared for the assault, he would have killed Randolph. He disliked the interference of friends who "interposed, closed the passage of the door, and held me, until I was oblige[d] to tell them if they did not open a passage I would open it with my cane."⁴⁴ In fact, when one friend offered to go out and kill Randolph immediately, the president rejected the offer: "I want no man to stand between me and my assailants, and none to take revenge on my account."⁴⁵ Several years later, after Jackson had left office and Randolph was finally apprehended for the assault, Jackson also rejected the

⁴¹ *Richmond Enquirer*, May 17, 1833; *National Intelligencer*, May 8, 1833.

⁴² *Richmond Enquirer*, May 17, 1833.

⁴³ *Richmond Enquirer*, May 14, 1833.

⁴⁴ Quoted in Robert V. Remini, *Andrew Jackson and the Course of American Democracy, 1833–1845* (New York, 1984), 61.

⁴⁵ Remini, *Andrew Jackson*, 61.

interference of the courts in what he regarded as an affair of honor. He asked President Van Buren to pardon Randolph. Such an act, he told Van Buren, would be in conformity with the wishes of his mother, who had long ago advised him, "[I]ndict no man for assault and battery or sue him for slander."⁴⁶ A personal attack on the body could only be countered by a return attack on the body.

There is another version of this episode hinted at in most newspaper stories, which did not linger on Jackson's spirited reaction to the assault. It went to the heart of the matter and focused on the one event that overwhelmed all others in significance. Andrew Jackson had had his nose pulled. This is what Randolph understood he had intended and accomplished,⁴⁷ for, as Randolph approached Jackson, he announced "[t]hat he came to take his revenge of him for the disgrace imposed upon him, by pulling his nose."⁴⁸ A few months later, during the dispute between Rives and Gilmer, a "Lieutenant Randolph outrage" had become another name for a nose pulling. Jackson himself, although denying that he had actually been touched, certainly understood that his nose was the object of the assault. In her autobiography, Peggy O'Neale—a woman with an extraordinary understanding of the minds of men of honor—recounted Jackson's sensitivity on the subject of his nose:

When the President was on visits to my father's, and I wished to tease him,—for I could take any of those childish liberties with him, as he regarded me only as a child—I would suddenly change whatever conversation was going on by looking upon the floor shaking my head mournfully, and saying, "Ah! General, it was very bad in you to let R. pull your nose." To this day I shake with laughter when I recall the violence with which he would always repel it. He invariably sprang to his feet, shook his fist, and said, "No; by the eternal God, madam, no man ever pulled my nose."⁴⁹

Perhaps not. But whether or not it actually happened, Jackson would not permit anyone to think that his nose had been pulled. He understood the full meaning of the act in the language of honor.

WHILE MEN WHO CARED ABOUT NOSES no doubt also dreamed about them, few recorded their nightmares. But one Southern man's nose nightmare did reach print in an 1835 issue of the *Southern Literary Messenger*.⁵⁰ It is the tale of a man who lies awake and restless, unable to sleep while crowded amidst other bodies on board a steam boat. Men with large noses snore loudly all around him as he ponders the prophecy of his Aunt Deborah that unless he learns to practice prudence and economy "his nose must come to the grindstone." Suddenly, two black slaves (men he recognizes as workers from a local tobacco factory) grab him by his arms and drag him to the deck of the ship, toward a grindstone being turned by a "black urchin." The slaves "forced my head downward, until my proboscis rested upon the revolving stone, and I felt its horrid inroads upon that sensitive member."⁵¹ While the wheel did its work, the slaves began to sing:

⁴⁶ Remini, *Andrew Jackson*, 61–62.

⁴⁷ Remini, *Andrew Jackson*, 61.

⁴⁸ *Richmond Enquirer*, May 14, 1833.

⁴⁹ Peggy Eaton, *The Autobiography of Peggy Eaton* (New York, 1932), 43–44.

⁵⁰ Pertinax Placid, "A Tale of a Nose," *Southern Literary Messenger*, 1 (April 1835): 445–48.

⁵¹ Placid, "Tale of a Nose," 447.

De man who hold his nose too high
Mus' be brought low:
Put him on de grinstone
And grind him off slow.

Wheel about, and turn about,
And wheel about slow;
And every time he wheel about
De nose must go.⁵²

The man of honor, held down by black slaves, slowly felt his nose disappear. "The friction of the stone upon my cheeks," he wrote, "gave fearful evidence that what had been a nose, existed no longer, and brought the horrid reflection that I was noseless! That the pride of my countenance was gone, and forever."⁵³ Here was a nightmare well understood by those who spoke the language of honor. It gave expression to their worst fears. For a Southern white man of honor to be deprived of his nose (with the added humiliation of having it removed by slaves) was to threaten his appearance and thus his very self.

⁵² Placid, "Tale of a Nose," 448.

⁵³ Placid, "Tale of a Nose," 448.

Class and State in Postemancipation Societies: Southern Planters in Comparative Perspective

STEVEN HAHN

AS SOUTHERN PLANTERS considered and debated their relationship to the federal union in the 1850s, the more thoughtful and historically conscious knew they had already been drawn into the swirl of a revolutionary age.¹ A scarce eight decades earlier, servile labor and slavery had predominated in Continental Europe and the Americas and served as the foundation of powerful landed elites. So widespread and enduring did social and economic dependence on these systems appear that early critics doubted that it could ever fully be dislodged. Then, amid a deepening crisis of colonial and monarchical regimes, the bonds of servility were steadily weakened, while the contours of political authority were refashioned. By the time the Confederate States of America was established, slavery and serfdom had been abolished in all but Cuba, Brazil, Puerto Rico, and Romania, and political revolutions had swept through much of the West. For it was an era of both emancipation and national unification and, as such, a moment of truth for the great landed classes.²

On the threshold of disunion, Southern planters had some cause to believe that the forces of historical change could be slowed, if not reversed. The aftermath of emancipation in St. Domingue and the British West Indies had led even former advocates of abolition to question the viability of free plantation labor.³ The

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¹ See, for example, James L. Roark, *Masters without Slaves: Southern Planters in the Civil War and Reconstruction* (New York, 1977), 14–23; Michael P. Johnson, *Toward a Patriarchal Republic: The Secession of Georgia* (Baton Rouge, La., 1977), 28–58; Eugene D. Genovese, *The World the Slaveholders Made: Two Essays in Interpretation* (New York, 1969), 118–244; Robert N. Olsberg, “A Government of Class and Race: William Henry Trescott and the South Carolina Chivalry, 1860–1865” (Ph.D. dissertation, University of South Carolina, 1972).

² Robin Blackburn, *The Overthrow of Colonial Slavery, 1776–1848* (London, 1988); Jerome Blum, *The End of the Old Order in Rural Europe* (Princeton, N.J., 1978); Eric Hobsbawm, *The Age of Revolution, 1789–1848* (New York, 1962); David Brion Davis, *The Problem of Slavery in the Age of Revolution, 1770–1823* (Ithaca, N.Y., 1975), 39–112; C. Duncan Rice, *The Rise and Fall of Black Slavery* (Baton Rouge, La., 1975); Seymour Drescher, “Brazilian Abolition in Comparative Perspective,” *Hispanic American Historical Review*, 68 (August 1988): 429. Although widely anticipated since the Crimean War, the abolition of Russian serfdom was decreed only one day after Jefferson Davis was inaugurated as president of the Confederacy.

³ Eric Foner, *Nothing but Freedom: Emancipation and Its Legacy* (Baton Rouge, La., 1983), 29–30; Joe B. Wilkins, “Window on Freedom: The South’s Response to the Emancipation of the Slaves in the British

revolutions of 1848 had, moreover, been defeated, and with them the radical and liberal parliamentary alternatives for nationhood in much of Western Europe. The storm, perhaps, was abating. Yet, in 1865, a short four years later, no such illusions could be entertained. The Confederacy had been crushed, slavery had been abolished, and much of the slaveholders' world lay in ruins. For decades thereafter, the planters and their heirs would complain about the extent and price of federal intervention in Southern affairs and about federal policies that favored other sections of the country and the classes within them.

The restoration of "home rule" in the South in the 1870s and the later work of Southern historians such as Ulrich B. Phillips, Robert S. Cotterill, and Wilbur J. Cash suggested that the war and Reconstruction were unfortunate and brief disjunctures in the course of Southern civilization and that a strong framework of continuity linked the Old South with the New.⁴ But their assessments had to compete with those of scholars whose purview was wider and who saw in the Civil War and Reconstruction a watershed, if not a revolution, for the South and the nation: Charles Beard and Mary Beard, W. E. B. Du Bois, Howard K. Beale, and C. Vann Woodward prominent among them. Woodward pronounced the most compelling verdict; indeed, a verdict so compelling that it defied the growing climate of consensus historiography in which it was rendered to achieve interpretive predominance. He argued that the planter class had been crippled and transformed, that the plantation system had been recast along capitalist lines, that a new elite based on cities and industry had risen, and, by the end of the nineteenth century, that this whiggish elite had become a junior partner in a national political economy ruled by Northern capital. However distinctive it may have remained compared with the North, the New South was truly new, and the old planters were no longer in command.⁵

Not until the late 1960s and early 1970s did the theme of continuity resurface in treatments of the postwar South. Reflecting the disappointments and failures of the "Second Reconstruction" and the need to explain the persisting problems of racism and underdevelopment, historians began to wonder whether the Civil War did in fact promote a fundamental transformation. And, asking searching questions and using sophisticated methods, many answered no. They insisted that federal officials helped resurrect the plantation economy, that Southern planters survived the turbulence of war and Reconstruction and reasserted their power, that repressive and coercive rural labor relations were maintained in guises other than slavery, and that the region's commercial and industrial classes remained weak. Some, informed by comparative perspectives and insights, claimed that the post-bellum South moved along a qualitatively different—"Prussian"—road of development from the North: it was modernization "from above" and specifically under the auspices, not

West Indies, 1833–1861" (Ph.D. dissertation, University of South Carolina, 1977); Alfred N. Hunt, *Haiti's Influence on Antebellum America: Slumbering Volcano in the Caribbean* (Baton Rouge, 1988).

⁴ Ulrich B. Phillips, "The Central Theme of Southern History," *AHR*, 34 (October 1928): 30–43; Robert S. Cotterill, "The Old South to the New," *Journal of Southern History*, 15 (February 1949): 3–8; W. J. Cash, *The Mind of the South* (New York, 1941). Also see C. Vann Woodward, *Thinking Back: The Perils of Writing History* (Baton Rouge, La., 1986), 62–63.

⁵ Charles A. Beard and Mary R. Beard, *The Rise of American Civilization*, 2 vols. (New York, 1933), 2: 52–121; Howard K. Beale, *The Critical Year: A Study of Andrew Johnson and Reconstruction* (New York, 1930); W. E. B. Du Bois, *Black Reconstruction in America, 1860–1880* (New York, 1935); C. Vann Woodward, *Origins of the New South, 1877–1913* (Baton Rouge, La., 1951). See also Sheldon Hackney, "Origins of the New South in Retrospect," *Journal of Southern History*, 38 (May 1972): 191–216.

of an emerging bourgeoisie but of a reactionary landed elite, with attendant social and political consequences.⁶

Challenging and useful, the "Prussian road" analogy exposes limitations and misconceptions that have found their way into what is a vibrant debate and flourishing literature on the transition from slavery to freedom and the character of the postwar South. For, whatever their disagreements, recent studies have, with a few notable exceptions,⁷ considered the matters of planter rule, social relations, class coalitions, and patterns of economic change almost exclusively from a regional, state, or local vantage point. The "Prussian road," on the other hand, is meant in large measure to describe the connection between class formation and modernization at the national level, much as did the work of the Beards, Beale, Du Bois, and Woodward.⁸ Together, they suggest that the experience of the post-bellum South and its planter class cannot be understood apart from the outcome of the preeminent political event of nineteenth-century America: the unification of the nation-state.

Few scholars doubt that the Civil War invested the American state with new power and authority over national life, however complex and prolonged the process of consolidating and implementing that power and authority proved to be.⁹ Yet, while political histories of the antebellum period usually integrate regional and national developments, the same cannot be said for the period that follows. Before the 1950s, scholars of postwar, especially post-Reconstruction, America hardly bothered with the South; during the past two decades, scholars of the South have bothered with little else. A readjustment is, therefore, very much in order, all the more so if both a national and international framework are employed and a

⁶ See, in particular, Jonathan M. Wiener, *Social Origins of the New South: Alabama, 1860–1885* (Baton Rouge, La., 1978); Jonathan M. Wiener, "Class Structure and Economic Development in the American South, 1865–1955," *AHR*, 84 (October 1979): 970–92; Jay R. Mandle, *The Roots of Black Poverty: The Southern Plantation Economy after the Civil War* (Durham, N.C., 1978); Dwight B. Billings, *Planters and the Making of a "New South": Class, Politics and Development in North Carolina* (Chapel Hill, N.C., 1979); Michael Schwartz, *Radical Protest and Social Structure: The Southern Farmers' Alliance and Cotton Tenancy, 1880–1890* (New York, 1976); Lewis N. Wynne, *The Continuity of Cotton: Planter Politics in Georgia, 1865–1890* (Macon, Ga., 1987); Carl N. Degler, *Place over Time: The Continuity of Southern Distinctiveness* (Baton Rouge, 1977). Although it is often attributed to Barrington Moore, Jr., the "Prussian road" concept was originally advanced by V. I. Lenin. Moore, in fact, never uses the term "Prussian road," referring instead to a "revolution from above." See V. I. Lenin, *The Development of Capitalism in Russia: The Process of the Formation of a Home Market for Large-Scale Industry* (1899; Moscow, 1974), 32–33; Barrington Moore, Jr., *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* (Boston, 1966), 228–313, 413–52.

⁷ See, for instance, Barbara J. Fields, "The Advent of Capitalist Agriculture: The New South in a Bourgeois World," in Thavolia Glymph and John J. Kushma, eds., *Essays on the Postbellum Southern Economy* (College Station, Tex., 1985), 73–94; Foner, *Nothing but Freedom*.

⁸ This is how Moore used a "revolution from above," although there is room for ambiguity. Lenin, for example, identified the Prussian, or landlord, path with the retention of large estates in the transition from serfdom to capitalist agriculture, and he counterposed it to the farmer, or American, path in which "all the relics of serfdom and large landownership" were destroyed by revolution. At the same time, Lenin saw a close relationship between the social and economic reorganization of the countryside and the deployment of class forces and political structures nationally. See Lenin, *Development of Capitalism in Russia*, 32–33. Also see Anthony Winson, "The 'Prussian Road' of Agrarian Development: A Reconsideration," *Economy and Society*, 11 (November 1982): 381–85.

⁹ Eric Foner, *Reconstruction: America's Unfinished Revolution, 1863–1877* (New York, 1988); Stephen Skowronek, *Building a New American State: The Expansion of National Administrative Capacities, 1877–1920* (Cambridge, 1982); Morton Keller, *Affairs of State: Public Life in Late Nineteenth-Century America* (Cambridge, Mass., 1977); Robert J. Kaczorowski, "To Begin the Nation Anew: Congress, Citizenship, and Civil Rights after the Civil War," *AHR*, 92 (February 1987): 45–68. There is disagreement over the extent and persistence of state and local prerogatives, which were not inconsiderable.

comparative analysis ventured—a venture that can benefit from an impressive body of new literature on the comparative history of emancipations.¹⁰

This essay on the national experience of the Southern planters in comparative perspective moves from the late antebellum era to the end of the nineteenth century, considers the position of the planters in relation to landed classes in the Western Hemisphere and Continental Europe before and, especially, after emancipation, looks at the association of class power and public policy, and draws more detailed comparisons with Brazil and Germany. Most of all, the essay points to the importance of political struggles for control of the resources and authority of the state.

IT HAS BECOME STANDARD PRACTICE for historians to write of the developing crisis of the slave South and its planter class during the antebellum period. With hindsight, this is surely appropriate. But it is equally important to recognize that, at the time, the planters of the Old South also stood as one of the most—if not the most—imposing of landed elites in the Western world. The sugar planters of the Caribbean may have owned, on average, much larger estates and many more slaves, and, although it had been thought that the British islands entered an era of prolonged decline from the 1770s on, recent research has painted a more sanguine portrait, at least for the period ending with the Napoleonic wars. Statistics on trade flows, prices, and profits tell only part of a still-controverted story, however; the other parts were clearly less auspicious.¹¹

Rampant absenteeism divided proprietors, whether in the British or French possessions, between those linked principally to the metropolis and those, frequently Creoles, who resided on their estates and often struggled for greater autonomy. Barbados, the Leeward Islands, Martinique, and Guadeloupe had been eclipsed, first by Jamaica and St. Domingue, then by Trinidad and Guyana. From the last quarter of the eighteenth century, growing competition generally made the

¹⁰ See, for example, Rebecca J. Scott, "Exploring the Meaning of Freedom: Postemancipation Societies in Comparative Perspective," *Hispanic American Historical Review*, 68 (August 1988): 407–28; Thomas C. Holt, "An Empire over the Mind: Emancipation, Race, and Ideology in the British West Indies and the American South," in J. Morgan Kousser and James M. McPherson, eds., *Region, Race, and Reconstruction: Essays in Honor of C. Vann Woodward* (New York, 1982), 283–313; C. Vann Woodward, "The Price of Freedom," in David Sansing, ed., *What Was Freedom's Price?* (Jackson, Miss., 1978), 93–113; Foner, *Nothing but Freedom*, 8–38; Stanley L. Engerman, "Economic Adjustments to Emancipation in the United States and British West Indies," *Journal of Interdisciplinary History*, 13 (Autumn 1982): 191–220; Fields, "Advent of Capitalist Agriculture."

¹¹ Until recently, scholars widely dated the economic decline of the British West Indies from the time of the American Revolution and argued that economic weakness helped prepare the way, first, for the abolition of the slave trade and, then, for emancipation. See, especially, Eric Williams, *Capitalism and Slavery* (Chapel Hill, N.C., 1944); Lowell Joseph Ragatz, *The Fall of the Planter Class in the British Caribbean, 1763–1833* (New York, 1928); Elsa V. Goveia, *Slave Society in the British Leeward Islands at the End of the Eighteenth Century* (New Haven, Conn., 1965); H. Orlando Patterson, *The Sociology of Slavery: An Analysis of the Origins, Development, and Structure of Negro Slave Society in Jamaica* (London, 1967). This view, and particularly Williams's work, has been challenged in relation to the abolition of the slave trade by Seymour Drescher's *Econocide: British Slavery in the Era of Abolition* (Pittsburgh, Pa., 1977). Drescher presented data pointing to the vitality of the sugar colonies in the imperial system through the Napoleonic wars and thus rejected the connection between economic decline and abolition. He has, in turn, provoked a spirited debate and serious questions about his dismissal of the "decline thesis." See, for example, Walter E. Minchinton, "Williams and Drescher: Abolition and Emancipation," *Slavery and Abolition*, 4 (September 1983): 81–100; Selwyn H. H. Carrington, "The American Revolution and the British West Indies Economy," *Journal of Interdisciplinary History*, 17 (Spring 1987): 823–50; Seymour Drescher, "The Decline Thesis of British Slavery since *Econocide*," *Slavery and Abolition*, 7 (May 1986): 3–21.

sugar colonies—especially the British—dependent on the favors of metropolitan governments for which they assumed diminishing importance. Nowhere were the planters sufficiently secure, economically, politically, or militarily, to establish their independence, despite regular grumbling and sporadic threats. They needed metropolitan naval and land forces to protect them both from foreign attack in an age of almost continuous European warfare and from their own restive slave populations. By the first decade of the nineteenth century, the West Indian lobby could no longer fend off the popular and parliamentary assault on the British slave trade; by the 1820s, whatever there was of a “golden age” in the British West Indies had passed, and a deepening crisis of debt, production, and labor unfolded. In St. Domingue during the 1790s, the most prosperous planters fell victim to the only successful slave revolt in modern history.¹²

In Brazil, the northeastern sugar economy, after 150 years of general expansion, entered a period of relative stagnation in the 1720s, with brief upturns thereafter occasioned by conflicts involving competitors in the Caribbean and by the Haitian revolution, which proved a boon to all other sugar-growing areas. The free colored population, an ever-present and substantial component of Brazilian society, was increasing rapidly at the expense of the slave population in the late colonial era. Between 1800 and 1835, Bahia was rocked by numbers of slave revolts in town and country alike.¹³ Although independence from Portugal was achieved in 1822 without armed struggle, political turbulence not only preceded the break but also attended an almost two-decade transition to a stable constitutional monarchy. During that time, the planter-mercantile elites squabbled among themselves as well as battled an autocratically inclined Emperor Dom Pedro II and small groups of urban-based radical liberals who evinced some antislavery sympathies. By 1840, the battle had largely been won. Yet, if the sugar planters (*senhores de engenho*) and their allies managed to survive without the popular mobilization and bloodshed that helped doom slavery on the Spanish-American mainland, they nonetheless had to share power with a monarch and contend with the British, who had secured commercial privileges and were waging a lengthy and determined effort to end the slave trade.¹⁴

The coffee boom of the mid-nineteenth century, spreading from the hinterlands of Rio de Janeiro into Minas Gerais and São Paulo, did breathe new life into the Brazilian export economy and the emerging south-central landed elite. But it further damaged the position of the northeastern sugar planters and promoted the

¹² Rice, *Rise and Fall of Black Slavery*, 155–57; Davis, *Problem of Slavery in the Age of Revolution*, 52–56; Blackburn, *Overthrow of Colonial Slavery*, 133–260; William A. Green, *British Slave Emancipation: The Sugar Colonies and the Great Experiment, 1830–1865* (Oxford, 1976), 35–64; Barry Higman, *Slave Population and Economy in Jamaica, 1807–1834* (Cambridge, 1976), 187–232; Thomas O. Ott, *The Haitian Revolution, 1789–1804* (Knoxville, Tenn., 1973), 3–21.

¹³ Stuart B. Schwartz, *Sugar Plantations in the Formation of Brazilian Society: Bahia, 1550–1835* (Cambridge, 1985), 160–201, 468–88; A. J. R. Russell-Wood, *The Black Man in Slavery and Freedom in Colonial Brazil* (New York, 1982), 50–82; Herbert S. Klein, “Nineteenth-Century Brazil,” in David W. Cohen and Jack P. Greene, eds., *Neither Slave nor Free: The Freedmen of African Descent in the Slave Societies of the New World* (Baltimore, Md., 1972), 309–34; João José Reis, *Rebelião escrava no Brasil: A história do levante dos malês, 1835* (São Paulo, 1986).

¹⁴ Emilia Viotti da Costa, *The Brazilian Empire: Myths and Histories* (Chicago, 1985), xvii–xx, 129–30; Emilia Viotti da Costa, “The Political Emancipation of Brazil,” in A. J. R. Russell-Wood, *From Colony to Nation: Essays on the Independence of Brazil* (Baltimore, Md., 1975), 43–88; José Murilo de Carvalho, *Teatro de Sombras: A Política Imperial* (Rio de Janeiro, 1988), 11–22, 50–61; James Lang, *Portuguese Brazil: The King's Plantation* (New York, 1979), 200–03; Blackburn, *Overthrow of Colonial Slavery*, 333–414; Leslie Bethell, *The Abolition of the Brazilian Slave Trade: Britain, Brazil, and the Slave Trade Question, 1807–1869* (Cambridge, 1970).

fragmentation of the planter class as a whole, accentuating a regionalism that would figure centrally in Brazil's political development. What is more, as Richard Graham has shown, coffee was weaker than cotton on the international market, leaving the coffee growers (*fazendeiros*) with comparatively less wealth than the planters of the American South, especially since their capital drain was greater.¹⁵

The nobilities of Continental Europe seem the epitome of the haughty and powerful landed elite, holding sway as they did over peasantries in various conditions of servility until the very late eighteenth century—and in many places well after that. Yet, during the last decades before emancipation, they faced serious challenges to their customary power and prerogatives, if not to their economic well-being. In many areas, they greeted calls for agricultural improvement with sufficient indifference or hostility that Jerome Blum could compare the rural economy of the “servile lands” during the eighteenth and early nineteenth centuries to “that of the undeveloped countries of the second half of the twentieth century.” Techniques and implements often remained traditional, capital accumulation was limited, and in the West and Russia absenteeism was widespread. Where the general price rise, dating from the 1760s, created opportunities and encouraged innovation, the nobles’ hold over the countryside was being loosened by the intrusions of bourgeois landowners and by peasant resistance to new seigneurial demands, while the nobles themselves devoted much of their surplus from the agricultural boom to consumption and borrowed heavily besides. As a consequence, social divisions and conflicts intensified and an escalating indebtedness “reached proportions that helped undermine the continued existence of the nobility as a special and privileged caste.”¹⁶

Politically, too, the nobility found themselves on the defensive, as pressure for agricultural reform added to the longstanding drive of absolutism for supremacy. The monarchs did not seek to overturn the rural order; they simply hoped to reduce noble prerogatives and burdens on the peasantry in order to achieve

¹⁵ Richard Graham, “Slavery and Economic Development: Brazil and the United States South in the Nineteenth Century,” *Comparative Studies in Society and History*, 23 (October 1981): 636–38; Celso Furtado, *The Economic Growth of Brazil* (Berkeley, Calif., 1971), 119–26. Along with the coffee areas of Rio, Minas, and São Paulo and the sugar areas of Bahia and Pernambuco, there was the mining sector of Minas Gerais, the cotton-producing Maranhão, the wheat and cattle-raising Rio Grande do Sul, and eventually the rubber-exporting Amazon. In the American South, on the other hand, the cotton boom of the early nineteenth century brought greater unity to the planter class.

¹⁶ Blum, *End of the Old Order*, 116–37, 155–70; B. H. Slicher Van Bath, *Agrarian History of Western Europe* (London, 1963), 103–13; Iván T. Berend and György Ránki, *East Central Europe in the Nineteenth and Twentieth Centuries* (Budapest, 1977), 17. Historians are beginning to show that Prussian Junkers frequently responded to the favorable market conditions of the last third of the eighteenth century by enclosing their demesnes, adopting improved crop rotations, and replacing forced with free wage labor. Nonetheless, this “transformation of manorial production occurred amidst a rising clamor of conflict between peasants and lords over labour dues and other seigneurial rents” and was accompanied by a growing crisis of estate indebtedness and the increasing importance of bourgeois owners, who may have claimed 10 percent of the estates by the turn of the nineteenth century. See William W. Hagen, “The Junkers’ Faithless Servants: Peasant Insubordination and the Breakdown of Serfdom in Brandenburg-Prussia, 1763–1811,” in Richard J. Evans and W. R. Lee, eds., *The German Peasantry: Conflict and Community in Rural Society from the Eighteenth to the Twentieth Centuries* (London, 1986), 71–101; Hartmut Harnisch, “Peasants and Markets: The Background to the Agrarian Reforms in Feudal Prussia East of the Elbe, 1760–1807,” in Evans and Lee, *German Peasantry*, 50–66; Hanna Schissler, “The Junkers: Notes on the Social and Historical Significance of the Agrarian Elite in Prussia,” in Robert Moeller, ed., *Peasants and Lords in Modern Germany* (Boston, 1986), 26–31. Hagen’s recent work has placed particular emphasis on the limits of Junker power and the importance of peasant resistance from the onset of the “second serfdom.” See William Hagen, “How Mighty the Junkers? Peasant Rents and Seigneurial Profits in Sixteenth Century Brandenburg,” *Past and Present*, 108 (August 1985): 80–116.

greater wealth and power.¹⁷ The independence of the nobility was most constrained in Russia, where privileges derived principally from civil and military service to the tsar rather than from direct control of estates. Indeed, although Russian nobles had won exclusive rights to land and serf ownership, over time, growing numbers of them were divorced from the land entirely.¹⁸ Even in Prussia, where Frederick the Great reversed the anti-Junker policies of his predecessors and strengthened the Junkers in the countryside, the Hohenzollerns had already created an impressive administrative system that included a bureaucracy increasingly self-conscious and restless, despite its continuing ties with the Junker-dominated military.¹⁹

The experience of the Old South's planters cannot fail to impress by comparison. At a time when the use of bound labor was coming under attack throughout the Atlantic world, the slave South underwent a spectacular geographic and economic expansion that thrust the chattel institution from the eastern seaboard out to Texas, helped unify the interests of the elite (chiefly as a result of the short-staple cotton boom), and created the largest slave population in the Western Hemisphere. Granted, slaveownership was broad based, slaveholding units normally were small, and even large-scale slaveholders paled by the standards of the Caribbean and Brazil. But Stuart Schwartz has recently demonstrated that small slaveholders predominated numerically almost everywhere and that slaveowning was more highly concentrated in the mid-nineteenth-century South than in the colonial Brazilian northeast.²⁰ More to the point, in the Lower South by 1860, farms and plantations containing at least twenty slaves accounted for well over 60 percent of the black labor force and over 60 percent of the cotton produced. In that year, the twelve wealthiest counties in America were to be found in the slave states, and, although the economic development of the South lagged behind that of the North, the Southern elite built an infrastructure and enjoyed a period of economic growth—largely because of the tremendous demand for cotton—that would have been the envy of any rural society in the world.²¹

At the same time, there were signs for concern. Over the course of the antebellum period, the planters' share of Southern wealth and slaves grew, albeit unevenly. In 1800, one-third of the white families in the region owned slaves; in

¹⁷ Blum, *End of the Old Order*, 197–281; Perry Anderson, *Lineages of the Absolutist State* (London, 1974), 15–59.

¹⁸ Terrence Emmons, *The Russian Landed Gentry and the Peasant Emancipation of 1861* (Cambridge, 1968), 3–18; Jerome Blum, "Russia," in David Spring, ed., *European Landed Elites in the Nineteenth Century* (Baltimore, Md., 1977), 68; Max Beloff, "Russia," in A. Goodwin, ed., *The European Nobility in the Eighteenth Century* (London, 1953), 172–89; Peter Kolchin, *Unfree Labor: American Slavery and Russian Serfdom* (Cambridge, Mass., 1987), 39–41, 58–61, 169.

¹⁹ A. Goodwin, "Prussia," in Goodwin, ed., *European Nobility*, 83–101; John R. Gillis, "Aristocracy and Bureaucracy in Nineteenth Century Prussia," *Past and Present*, 41 (December 1968): 111; Theda Skocpol, *States and Social Revolution: A Comparative Analysis of France, Russia, and China* (Cambridge, 1979), 104–09.

²⁰ Stuart B. Schwartz, "Patterns of Slaveholding in the Americas: New Evidence from Brazil," *AHR*, 87 (February 1982): 55–86. Schwartz contrasted the slaveholding patterns in Bahia and the U.S. South with Jamaica, where most of the slaves lived on units of 100 or more. But his data also show that nearly 70 percent of Jamaican slaveholders—and more than 75 percent of Southern and Bahian slaveholders—owned fewer than ten slaves.

²¹ James D. Foust, *The Yeoman Farmer and the Westward Expansion of United States Cotton Production* (New York, 1975), 115–18; Robert Fogel and Stanley Engerman, *Time on the Cross: The Economics of American Negro Slavery*, 2 vols. (Boston, 1974), 1: 59–106; James Oakes, *The Ruling Race: A History of American Slaveholders* (New York, 1982), 39; Gavin Wright, *The Political Economy of the Cotton South: Households, Markets, and Wealth in the Nineteenth Century* (New York, 1978), 89–97; Stanley L. Engerman, "A Reconsideration of Southern Economic Growth, 1790–1860," *Agricultural History*, 49 (April 1975): 343–61; Graham, "Slavery and Economic Development," 620–55.

1860, only one-quarter did, and social stratification seemed more pronounced. During the 1850s, some plantation districts saw growing numbers of propertyless white laborers, while larger towns and cities witnessed a significant influx of European immigrants, who soon dominated the skilled trades and resented slave competition. Slaveholders and their spokesmen thus turned more attention, in speeches and in print, to explaining "the interest of nonslaveholders" in the slave regime. Some warned of imminent class and racial conflict. Despite such trends, until secession itself, the Southern planter class presided over the most stable and prosperous servile order in modern history.²²

In the political arena, notwithstanding an increasing defensiveness during the antebellum period, the Southern landed elite retained both a remarkable degree of control over its own affairs and a strong voice in national politics. The planters were the first slaveholding class in the hemisphere to gain independence; they did not have a monarch with whom to contend over labor policies or on whom they had to depend for financial support; they helped write a constitution that not only sanctioned slavery while limiting federal jurisdiction over it but also rewarded slaveholders with more representation than other Americans; and they had no need for the standing armies that sapped the resources and talents of their counterparts in war-torn Europe. In fact, freedom from British imperial restraints and a subsequent loose federalism opened the way for the westward migration that bolstered the slave system.²³ Until 1850, Southerners held the presidency for all but thirteen years, occupied half the seats in the Senate, and wielded great power in the House of Representatives as a result of their influence in the Democratic party. The South thereby managed to circumscribe federal authority and unfavorable economic policies. When the free states came to outnumber the slave in the 1850s, Southern interests gained protection from sympathetic Northern Democrats in the White House, even greater control of the Democratic party, and a decisive majority on the Supreme Court. Before 1860, nearly 60 percent of all Supreme Court justices were Southerners, and the South had parity or, more often, a majority on

²² Wright, *Political Economy of Cotton South*, 29–37; J. William Harris, *Plain Folk and Gentry in a Slave Society: White Liberty and Black Slavery in Augusta's Hinterlands* (Middletown, Conn., 1985), 64–93; Steven Hahn, *The Roots of Southern Populism: Yeoman Farmers and the Transformation of the Georgia Upcountry, 1850–1890* (New York, 1983), 15–49, 86–91; Joseph P. Reidy, "Masters and Slaves, Planters and Freedmen: The Transition from Slavery to Freedom in Central Georgia, 1820–1880" (Ph.D. dissertation, Northern Illinois University, 1982), 64–77; Stephanie McCurry, "Defense of Their World: Gender, Class, and the Yeomanry of the South Carolina Lowcountry, 1820–1860" (Ph.D. dissertation, State University of New York, Binghamton, 1988), 12–81; William L. Barney, "Toward the Civil War: The Dynamics of Change in a Black Belt County," in Orville Vernon Burton and Robert C. McMath, Jr., eds., *Class, Conflict, and Consensus: Antebellum Southern Community Studies* (Westport, Conn., 1982), 146–65; James C. Bonner, "Profile of a Late Antebellum Community," *AHR*, 49 (July 1944): 663–80; Ira Berlin and Herbert G. Gutman, "Natives and Immigrants, Free Men and Slaves: Urban Workingmen in the Antebellum American South," *AHR*, 88 (December 1983): 1175–1200. The rice economy of the South Carolina Low country, as Peter Coclanis argues, had been in relative decline for about three decades on the eve of the Civil War, as a result of competition from East Indian producers. Nonetheless, rice planters exported a record crop in 1859 and remained among the true grandees of the antebellum South. See Coclanis, *The Shadow of a Dream: Economic Life and Death in the South Carolina Low Country, 1670–1920* (New York, 1989), 111–58.

²³ John R. Alden, *The First South* (Baton Rouge, La., 1961), 74–133; Donald L. Robinson, *Slavery in the Structure of American Politics, 1765–1820* (New York, 1971); David Brion Davis, "American Slavery and the American Revolution," in Ira Berlin and Ronald Hoffman, eds., *Slavery and Freedom in the Age of the American Revolution* (Charlottesville, Va., 1983), 273–74. According to Gordon A. Craig, the decimation of the Junker-dominated officer corps during the Seven Years' War proved a serious blow and helped pave the way for disaster in 1806. See Craig, *The Politics of the Prussian Army, 1640–1945* (New York, 1964), 24–25.

the court for all but eleven years between the ratification of the Constitution and the onset of the Civil War.²⁴

Historians frequently allude to the democratic character of antebellum Southern politics (for white men) to distinguish the tendencies and world-view of the planter class from landed elites elsewhere in the Americas and Europe. There is no denying the contention; Southern planters were not culturally akin to Brazilian sugar planters, Prussian Junkers, or Russian noble landowners (*pomeshchiki*), although the work of Shearer Davis Bowman and Peter Kolchin suggests important similarities.²⁵ Nonetheless, the Southern political system long exhibited patron-client features that were, in several respects, broadened and reinforced by the tide of Jacksonian democratization, buttressing the hegemony of the planter class. Above all, we must recognize the conservative and anti-democratic trajectory of the slave regime as a whole, as well as the new dimensions of struggle for state power that the sectional conflict encouraged. Many Northerners certainly did. The suppression of abolitionist literature, censorship of the mails, the enactment of the gag rule and a tougher fugitive slave law, the decision in the Dred Scott case, and the demand for federal protection for slavery in the territories did more than signal the South's increasingly reactionary role in national life. They also made plain that the South, in a shift of stance, sought to use the national government for its own purposes and that the maintenance of slavery would commit federal resources to socially and politically repressive policies. Northerners feared the expanding "slave power," and with good reason: politically prominent Southerners called for the nullification of all personal liberty laws in the North, and there are grounds to believe that, were it not for secession, the Supreme Court might have effectively "nationalized" slavery by invalidating all obstacles to slave transit.²⁶

IT IS IN LIGHT OF THE STRENGTH of the Southern landed elite in national affairs and the histories of other landed classes that the unfolding of emancipation and unification in the United States must be considered. What then becomes strikingly apparent is not the persistence of the Southern planters' power but rather its dramatic and irreversible decline. After all, the South was not alone among servile

²⁴ Harold Chase, et al., *Biographical Dictionary of the Federal Judiciary* (Detroit, Mich., 1976). See also David M. Potter, *The South and the Concurrent Majority* (Baton Rouge, La., 1972), 3–28; William J. Cooper, *The South and the Politics of Slavery, 1828–1856* (Baton Rouge, 1978).

²⁵ Shearer Davis Bowman, "Antebellum Planters and Vormärz Junkers in Comparative Perspective," *AHR*, 85 (October 1980): 779–808; Peter Kolchin, "In Defense of Servitude: American Proslavery and Russian Proserfdom Arguments, 1760–1860," *AHR*, 85 (October 1980): 808–27; Kolchin, *Unfree Labor*, 49–191 and *passim*. The similarities included a general social and political conservatism as well as the relatively modest size of Junker estates and Southern slave plantations. For discussion of a democratic political culture in the antebellum South, see J. Mills Thornton III, *Politics and Power in a Slave Society: Alabama, 1800–1860* (Baton Rouge, La., 1978); Lacy K. Ford, *Origins of Southern Radicalism: The South Carolina Upcountry, 1800–1860* (New York, 1988); Oakes, *Ruling Race*; George M. Fredrickson, *The Black Image in the White Mind: The Debate on Afro-American Character and Destiny, 1817–1914* (New York, 1971), 58–70.

²⁶ Eugene D. Genovese, "Slavery: The World's Burden," in Harry P. Owens, ed., *Perspectives and Irony in American Slavery* (Jackson, Miss., 1976), 38–39; Paul Finkelman, *An Imperfect Union: Slavery, Federalism, and Comity* (Chapel Hill, N.C., 1981), 313–43 and *passim*. Much remains to be done on patron-client politics in the Old South, but for some suggestive material, see Charles Sydnor, *American Revolutionaries in the Making* (New York, 1965); Elizabeth Fox-Genovese and Eugene D. Genovese, *Fruits of Merchant Capital: Slavery and Bourgeois Property in the Rise and Expansion of Capitalism* (New York, 1983), 249–64; Hahn, *Roots of Southern Populism*, 91–105; D. Alan Williams, "The Small Farmer in Eighteenth Century Virginia Politics," *Agricultural History*, 43 (January 1969): 92–101; McCurry, "Defense of Their World," 413–63.

societies in resisting emancipation, however much secession and civil war lent it a distinctive aspect. No landed elite easily accepted emancipation. Recalcitrance prevailed at the very least; violence, upheaval, or military crisis normally occurred before the deed was done. There can be no separating the abolition of slavery and other servile relations in the West from the great Christmas uprising of 1831 in Jamaica, the Ten Years' War in Cuba, the Paraguayan War and slave flight from the plantations in Brazil, the revolutions of 1789 and 1848 in France, the Napoleonic wars and the Revolution of 1848 in Prussia, the Crimean War in Russia, and peasant unrest in many parts of the Continent.²⁷ Still, aside from St. Domingue, no landed elite was wholly toppled by the emancipation process; in most areas—the South included—various forms of extra-economic compulsion, both new and old, continued to shape rural labor relations for some time thereafter.

Only in the United States and St. Domingue, however, did abolition come in the midst of a massive shift in power away from the landed classes. Elsewhere, their objections, protests, and threats notwithstanding, the elites were able to define the boundaries of emancipation policy and supervise its implementation. Either they were compensated for their losses or awarded state subsidies to attract new labor and buffer the transition. In any event, they retained direct control over the countryside and wide prerogatives to regulate freedpersons. In the British Caribbean, the planters received a share of the £20,000,000 distributed throughout the empire, a temporary apprenticeship system, and a large measure of home rule. In Brazil, the coffee growers benefited from a state-financed program to import free workers even before slavery ended. And, in Europe, landed proprietors extracted indemnification from the peasantry that often delayed by years the peasants' full emancipation.²⁸

Whatever the shortcomings of federal emancipation and reconstruction policy, in the United States alone it was formulated without the landed elite's direct influence, and, as a consequence, proved the most sweeping and far-reaching.²⁹ Nowhere else were so many servile laborers liberated in one stroke or soon after provided equivalent civil and political rights. Indeed, for some time, it seemed that a major program of land reform might be initiated. Even with the failure of land reform, black enfranchisement and the active organizing of the Republican party presented a serious political challenge to planters who had already suffered military defeat and heavy economic losses. The freedmen pressed their claims, came to sit in Southern state legislatures, and—in numerous plantation belt counties—took

²⁷ Michael Craton, *Testing the Chains: Resistance to Slavery in the British West Indies* (Ithaca, N.Y., 1982), 291–321; Genovese, *World the Slaveholders Made*, 25–95; Blackburn, *Overthrow of Colonial Slavery*, 421–68, 507–12; Blum, *End of the Old Order*, 332–76; Rebecca J. Scott, *Slave Emancipation in Cuba: The Transition to Free Labor, 1860–1899*, (Princeton, N.J., 1985), 45–62.

²⁸ Foner, *Nothing but Freedom*, 8–38; Green, *British Slave Emancipation*, 99–161; Rice, *Rise and Fall of Black Slavery*, 261–62; Emilia Viotti da Costa, *Da Senzala à Colônia* (São Paulo, 1966), 188–202; Michael M. Hall, "The Origins of Mass Immigration in Brazil, 1871–1914" (Ph.D. dissertation, Columbia University, 1969), 81–115; Ademir Gebara, *O Mercado de trabalho livre no Brasil (1871–1888)* (São Paulo, 1986), 84–91; Blum, *End of the Old Order*, 377–417. On the Cuban case, see Scott, *Slave Emancipation*, 201–26.

²⁹ The development of emancipation policy was much affected by the need to placate slaveholders in the strategic border states that did not join the Confederacy. The literature on this subject is large, but for the most recent and penetrating account, see Ira Berlin, Barbara J. Fields, Thavolia Glymph, et al., eds., *Freedom: A Documentary History of Emancipation*; ser. 1, vol. 1, *The Destruction of Slavery* (Cambridge, 1985).

command of the local government. To many whites, it appeared nothing less than a world turned upside down.³⁰

The radical changes and inversions did not endure. A massive counteroffensive, made possible by a limited and flagging federal commitment to revolutionizing the South, rolled back many of the freedmen's gains, rapidly in some areas and gradually in others. And as a growing number of local studies are demonstrating—studies that have contributed to various “continuity” theses—the planters did maintain or reassert their dominance over much of the Southern countryside. Evidence from virtually every ex-Confederate state shows that the rate at which plantation owners persisted from 1860 to 1870 differed little from the previous decade and that the landed elite of the 1870s had firm roots in the antebellum era. No doubt a considerable turnover in land titles took place during the postwar years, and we need to learn more about its extent. Even so, its implications are not readily obvious: very high turnover rate among Junker estates in parts of mid-nineteenth-century Prussia, for example, failed to bring about a significant weakening of traditional landed power.³¹ Capital from the North and abroad did make its way into Southern agriculture, though not for some time after emancipation, and it went chiefly into the prairie rice, sugar, and Mississippi Delta cotton sectors. Merchandisers, for the most part Southern-born and filling the vacuum left by the eroding factorage system, did begin to establish a strong foothold in rural districts and move into the ranks of substantial landed proprietors, although they did so most prominently in heavily white, nonplantation counties. The Black Belt, from Virginia to Texas, by and large saw not so much the displacement as the bourgeois metamorphosis of older planter families and their holdings.³²

Although we still know less about the social bases of Southern politics after the war than before it, there is also good reason to believe that the landed elite retained the upper hand, albeit uneasily, into the twentieth century. Thrown on the defensive by freedmen, merchants, industrial interests, and Republicans generally during Reconstruction, planters fended off its worst effects and led the movement

³⁰ Woodward, “Price of Freedom,” 93–113; Foner, *Reconstruction*, 228–345. In France, all seigneurial obligations were terminated without indemnification between 1789 and 1793, and, because of the confiscation of land held by the church and *émigrés*, some peasants were able to add to their holdings. Nonetheless, seigneurs often could use rent increases to compensate themselves for the loss of servile obligations. It is difficult to determine how many peasants were “emancipated” during the revolutionary upheavals, for seigneurial obligations varied considerably; enfranchisement, however, largely had to wait until the Second Republic. See Blum, *End of the Old Order*, 394, 398; P. M. Jones, *The Peasantry in the French Revolution* (Cambridge, 1988); Theodore S. Hamerow, *The Birth of a New Europe: State and Society in the Nineteenth Century* (Chapel Hill, N.C., 1983), 305–06.

³¹ David Spring, “Landed Elites Compared,” in Spring, ed., *European Landed Elites*, 4–5. On planter persistence, see Wiener, *Social Origins of the New South*, 3–34; Michael S. Wayne, *The Reshaping of Plantation Society: The Natchez District, 1860–1880* (Baton Rouge, La., 1983); Crandall A. Shiflett, *Patronage and Poverty in the Tobacco South: Louisa County, Virginia, 1860–1900* (Knoxville, Tenn., 1982), 3; Stephen V. Ash, *Middle Tennessee Society Transformed, 1860–1870: War and Peace in the Upper South* (Baton Rouge, 1988), 226–32; Randolph B. Campbell, *A Southern Community in Crisis: Harrison County, Texas, 1850–1880* (Austin, Tex., 1983), 382–91; Kenneth S. Greenberg, “The Civil War and the Redistribution of Land: Adams County, Mississippi, 1860–1870,” *Agricultural History*, 52 (April 1978): 292–307.

³² Pete Daniel, *Breaking the Land: The Transformation of Cotton, Tobacco, and Rice Cultures since 1880* (Urbana, Ill., 1985), 39–61; Woodward, *Origins of the New South*, 114–20, 179; Hahn, *Roots of Southern Populism*, 137–203; James C. Cobb, “Beyond Planters and Industrialists: A New Perspective on the New South,” *Journal of Southern History*, 54 (February 1988): 49–50; Robert L. Brandfon, *Cotton Kingdom of the New South: A History of the Yazoo Mississippi Delta from Reconstruction to the Twentieth Century* (Cambridge, Mass., 1967). Gavin Wright, acknowledging the persistence of planter families, described this bourgeois metamorphosis as a transition from laborlords to landlords. See Wright, *Old South, New South: Revolutions in the Southern Economy since the Civil War* (New York, 1986), 47–50. Also see Fields, “Advent of Capitalist Agriculture,” 79–81.

to overthrow Republican regimes. Redeemer constitutions of the 1870s then restricted state-supported economic development, while state legislatures of the period enacted a variety of measures that strengthened the hands of landlords, amplified the voice of rural counties, established state agricultural departments, provided tax benefits to plantation districts, and brought railroad companies under scrutiny. Dissension continued to strain the Democratic party, with its unwieldy factions; but, within the shifting party coalitions, the large landowners usually functioned as the anchors.³³ In short, it is difficult to argue that the post-bellum South came under the control of a new urban and industrial ruling class.

From a national and international vantage point, however, the position of the postwar Southern landed elite appears far less sanguine, and the dominant political bloc coming to govern the country far more exceptional. In the United States, as in other Western countries, emancipation helped usher in the final stages of national unification and nation building: the absorption of regional elites and labor systems into capitalist nation-states. In this regard, the Civil War and Reconstruction formed part of a much larger socio-political process that completed what Eric Hobsbawm has called "the global triumph of capitalism."³⁴ Yet the process was by no means uniform in character, pace, or outcome; the examples of Brazil and Germany highlight important contrasts with America.

The Brazilian case presents some particularly novel features when compared with the United States. To begin with, slavery prevailed throughout the country. As late as the 1870s, every province had its slaves, and on the eve of emancipation all but northernmost Amazonas and Ceará still held them.³⁵ Furthermore, the establishment of the First Republic in 1889, almost immediately after abolition, brought about political and administrative decentralization, the reverse of the trend almost everywhere else. What linked these features was the nature of the class and region assuming the dominant position in the new Brazilian state.

Between the achievement of independence and the onset of the republic, Brazil was held together, tenuously at times, by a constitutional monarchy that concentrated policy making in the hands of the monarch (known as the emperor) and the institutions (appointive and elective) that oversaw the imperial system. Although the emperor ultimately depended on the support of the rural slaveholding elites and could seldom enforce his will on questions of national importance, he nonetheless had to perform a difficult balancing act: juggling the demands of

³³ See, for example, Michael Perman, *The Road to Redemption: Southern Politics, 1869–1879* (Chapel Hill, N.C., 1984), 178–277; Foner, *Reconstruction*, 587–601; William J. Cooper, *The Conservative Regime: South Carolina, 1877–1890* (Baltimore, Md., 1968); Allen J. Goings, *Bourbon Democracy in Alabama, 1874–1890* (University, Ala., 1951); Alwyn Barr, *Reconstruction to Reform: Texas Politics, 1876–1906* (Austin, Tex., 1971); Roger L. Hart, *Redeemers, Bourbons, and Populists: Tennessee, 1870–1896* (Baton Rouge, La., 1975); Peter Wallenstein, *From Slave South to New South: Public Policy in Nineteenth-Century Georgia* (Chapel Hill, 1987), 187–90; Wynne, *Continuity of Cotton*; Michael Hyman, "Response to Redeemer Rule: Hill Country Political Dissent in the Post-Reconstruction South" (Ph.D. dissertation, City University of New York, 1986), 196–259; Willie D. Halsell, "The Bourbon Period in Mississippi Politics, 1875–1890," *Journal of Southern History*, 11 (November 1945): 529–31; James Tice Moore, "Redeemers Reconsidered: Change and Continuity in the Democratic South," *Journal of Southern History*, 44 (August 1978): 357–78. For discussions of challenges to the political power of the landed elite during the post-Reconstruction period, see Randolph Werner, "Hegemony and Conflict: The Political Economy of a Southern Region, Augusta, Georgia, 1865–1895" (Ph.D. dissertation, University of Virginia, 1977); Sheldon Hackney, *Populism to Progressivism in Alabama* (Princeton, N.J., 1969); Jack P. Maddex, *The Virginia Conservatives, 1867–1879: A Study in Reconstruction Politics* (Chapel Hill, 1970); Hahn, *Roots of Southern Populism*, 204–89.

³⁴ Eric J. Hobsbawm, *The Age of Capital, 1848–1875* (New York, 1979), xvii.

³⁵ Robert Conrad, *The Destruction of Brazilian Slavery, 1850–1888* (Berkeley, Calif., 1972), 284–85; da Costa, *Brazilian Empire*, 166–67.

troubled sugar planters in the northeast, rising coffee growers of the south-central region, Portuguese-born merchants and courtiers, a small urban middle class, and the abolition-minded British, who also played a pivotal role in the export economy. Local revolts, which flared sporadically before 1850, testified to the tensions within the empire. Relative peace then followed until the American Civil War and, especially, the traumatic Paraguayan War (1865–1870) gave impetus to an indigenous, urban-based antislavery movement that viewed abolition as a necessary step toward modernization. While Emperor Dom Pedro II comprehended its political complexities, his sentiments leaned toward the side of emancipation reform.³⁶

Few Brazilian slaveholders unequivocally embraced abolitionism. They proved least reluctant in the northeast, where slavery had been in long-term decline as a result of both manumissions and the internal slave traffic, although they expected emancipation to be gradual and bring compensation. Far more resistance could be found in the south, notably in the Paraíba valley, where the coffee economy was expanding and heavily dependent on slave labor. Even the Paulista planters, whose rise began as the Atlantic slave trade terminated and the slavery crisis escalated, showed little desire to hasten slavery's demise.³⁷ Yet, as early as the 1840s, some Paulista planters experimented, for the most part unsuccessfully, with free labor. By the 1880s, they had enlisted the aid of the provincial and imperial governments in financing an immigrant labor program. Thousands of workers began to stream in, and by May 1887 recently arrived Italians already outnumbered slaves working on Paulista coffee plantations (*fazendas*). Seven months later, as large numbers of fleeing slaves prompted some owners to grant freedom in return for continued labor, the most prosperous planters accepted abolition. Shortly thereafter, in 1888, the more-than-three-century reign of slavery came to an end.³⁸

Having emerged as the most vital of Brazilian regions by the 1870s, São Paulo suffered under a centralized regime in which its political power was not commensurate with its economic power. While the province's income increased rapidly, it remained grossly underrepresented in the ministries and council of state as well as in the senate. A budding urban bourgeoisie and the growth of manufacturing further exposed the outmoded quality of the imperial structure and helped to

³⁶ C. H. Haring, *Empire in Brazil: A New World Experiment with Monarchy* (New York, 1968), 18–62; da Costa, *Brazilian Empire*, 216–20; da Costa, "Political Emancipation of Brazil," 77–88; Richard Graham, *Britain and the Onset of Modernization in Brazil, 1850–1914* (Cambridge, 1968), 23–176; Robert Brent Toplin, *The Abolition of Slavery in Brazil* (New York, 1972), 38–130; Murilo de Carvalho, *Teatro de Sombras*, 50–83; Conrad, *Destruction of Slavery*, 70–89; Rebecca B. Bergstresser, "The Movement for the Abolition of Slavery in Rio de Janeiro, Brazil, 1880–1889" (Ph.D. dissertation, Stanford University, 1973).

³⁷ Conrad, *Destruction of Slavery*, 151–69; Toplin, *Abolition of Slavery*, 9–17, 131–44; Peter L. Eisenberg, *The Sugar Industry in Pernambuco, 1840–1910: Modernization without Change* (Berkeley, Calif., 1974), 166–70; Stanley Stein, *Vassouras: A Brazilian Coffee County, 1850–1900* (Cambridge, Mass., 1957). On social, political, and cultural concerns surrounding abolition, see Celia Maria Marinho de Azevedo, *Onda negra, medo branco: O Negro no imaginário das elites—século XIX* (Rio de Janeiro, 1987).

As early as 1871, Brazil embarked on a course of legislated gradualism when the Rio Branco Law provided for the eventual freedom of children born of slave mothers. In 1885, the Sexagenarian Law provided for the liberation of the elderly. But these laws did not have a great impact on the size of the slave population. See Peter L. Eisenberg, "Abolishing Slavery," *Hispanic American Historical Review*, 52 (November 1972): 580–97.

³⁸ Warren Dean, *Rio Claro: A Brazilian Plantation System, 1820–1920* (Stanford, Calif., 1976), 88–123; da Costa, *Brazilian Empire*, 125–71; Joseph L. Love, *São Paulo in the Brazilian Federation, 1889–1937* (Stanford, 1980), 8–10; Gebara, *O Mercado de trabalho livre*, 121–99; Hall, "Origins of Mass Immigration," 81–111. The trend toward free labor on the plantations was well advanced in the northeast much earlier, and in the 1870s free people of color were joined by denizens of the backlands who were forced by drought to look for employment in the sugar areas. In Pernambuco by 1872, free workers outnumbered slaves in both skilled and unskilled occupations.

inspire a republican movement. But opposition to the monarchy intensified as abolitionists pressed for land reform as an accompaniment to emancipation. For the Paulista planters, attracted by the idea of federalism, also feared that a weakened emperor and a chaotic political system might be unable to resist such a radical assault. Thus the coffee growers, especially the more advanced section in the west, sided with the republicans and were ultimately joined by northeastern sugar planters disgruntled over the empire's failure to provide indemnification for their liberated slaves. Once the military abandoned the monarchy, its days were numbered; a bloodless coup toppled it in 1889.³⁹

The ensuing federal republic greatly improved the position of São Paulo and its coffee planters, while securing the power of rural elites in less developed areas. It thereby revealed the decisive imprint of landed wealth and export agriculture. On the one hand, each of the twenty Brazilian states received considerable measures of self-government, including the right to tax exports and borrow abroad, which worked to the advantage of São Paulo and Minas Gerais, the two major coffee-producing states, as well as the sugar-growing northeast. On the other hand, the national government itself soon came under the sway of an alliance between São Paulo and Minas by virtue of an agreement that granted state governors and bosses essential autonomy over their own domains in return for Paulista and Mineiro control over the presidency and other strategic federal posts. As a result, although their will did not go uncontested and the implementation of policy could require extended negotiation, the coffee planters collected sufficient revenues to propel the growth of their own states and won federal support to prop up the export economy. Not only was further assistance for immigrant labor forthcoming but, when coffee prices slumped in the late 1890s, a federal-state valorization scheme was fashioned whereby government-secured foreign loans underwrote the stockpiling of coffee for more opportune sale, a program that buoyed the planters in choppy market waters for the next two decades.⁴⁰

So it was that, in the course of emancipation and nation-building, the landed classes throughout Brazil retained their property, control over labor, and local prerogatives. The coffee planters, furthest along the road of capitalist agriculture, also succeeded in using the resources of the state to advance their interests. The overwhelmingly immigrant urban bourgeoisie, despite some real gains, failed to establish an independent base: they aligned with the coffee planters, who in turn spearheaded much of the industrial investment. Indeed, the substantial industrial growth experienced by São Paulo in the late nineteenth and early twentieth centuries came chiefly under the auspices of a class whose major economic foundation was export agriculture and whose economic policies served to weaken

³⁹ Da Costa, *Brazilian Empire*, 202–33; Richard Graham, "Landowners and the Overthrow of the Empire," *Luso-Brazilian Review*, 7 (December 1970): 44–56; Haring, *Empire in Brazil*, 144–56; Toplin, *Abolition of Slavery*, 248–56.

⁴⁰ See Barbara Weinstein, "Brazilian Regionalism," *Latin American Research Review*, 17 (1982): 262–76; Love, *São Paulo*, 44–48; Warren Dean, *The Industrialization of São Paulo, 1880–1945* (Austin, Tex., 1969), 44; Peter L. Eisenberg, "The Consequences of Modernization for Brazil's Sugar Plantations in the Nineteenth Century," in Ian Rutledge and Kenneth Duncan, eds., *Land and Labour in Latin America: Essays on the Development of Agrarian Capitalism in the Nineteenth and Twentieth Centuries* (Cambridge, 1977), 350; and especially Steven Topik, *The Political Economy of the Brazilian State, 1889–1930* (Austin, Tex., 1987). The success of the valorization program depended in large part on the dominant position of Brazilian coffee on the international market—for the nation accounted for 80 percent of world production—as well as the relative inelasticity of a burgeoning demand. But the program also reflected the power of the coffee planters, who won out in the face of wide-ranging opposition: not only from the producers of other exports or of commodities consumed within Brazil but also from growers of lower quality beans, who did not benefit.

federal finances, inhibit the importation of essential machinery, deny other exporting regions much-needed capital, and impede the expansion of the domestic market. The price would be limited economic development for the nation as a whole and increased foreign influence over the financial sector.⁴¹

THE CONTOURS OF GERMAN UNIFICATION bear closer resemblance to those of the United States, although many of the aspects and certainly the outcome were vastly different. Like the United States and unlike Brazil, the domains of landed estates and industrial capital were distinct geographically, and the unification process resulted in greater political centralization and economic development. At the same time, the commercial and industrial classes, albeit not weak and dependent as in Brazil, had a more limited base and experienced more difficulties than their counterparts in the United States. On balance, the territorial, demographic, and political factors favored Old Prussia, even as the Hohenzollern monarchy and civil bureaucracy curbed the power of resident Junkers, especially before 1815.⁴²

The reforms of the early nineteenth century, in particular the emancipation decrees, along with the acquisition of the Rhineland after the Napoleonic wars, set the final forces of unification in motion. Humbled by the French armies at Jena and encouraged by reform-minded civil servants, the Prussian king and his new chief minister, Baron vom Stein, abolished hereditary serfdom and declared land a free commodity in 1807, despite strong Junker protests. Nevertheless, the peasants were left with a variety of servile dues and the burdens of indemnification, and, when the reformers began to challenge other noble prerogatives, they were dismissed by the crown. The Junkers retained—indeed, were strengthened in—their control of the countryside and remained exempt from land taxes.⁴³ Only when the upheavals of 1848 were felt throughout the Prussian state were all the bonds of peasant servility severed. By then, the Junkers had annexed thousands of peasant holdings, leaving a mass of landless and land-poor agricultural laborers to work their estates alongside growing numbers of Polish migrants.⁴⁴

⁴¹ Dean, *Industrialization of São Paulo*, 49–78; Love, *São Paulo*, 53–67; Topik, *Political Economy of the Brazilian State*, 69–74; Weinstein, “Brazilian Regionalism,” 274–75; Wilson Cano, *As raízes da concentração industrial em São Paulo* (São Paulo, 1979), 42–87, 121–94.

⁴² Anderson, *Lineages of the Absolutist State*, 236–73; James J. Sheehan, “Conflict and Cohesion among German Elites in the Nineteenth Century,” in James J. Sheehan, ed., *Imperial Germany* (New York, 1976), 65–70; Gillis, “Aristocracy and Bureaucracy,” 105–13. As Kenneth Barkin wrote, “although Germany, like the United States, was composed of numerous states, an essential difference lay in the dominant position held by Prussia . . . Every conceivable criterion pointed to the ascendancy of Prussian power in Germany.” See Barkin, *The Controversy over German Industrialization, 1890–1902* (Chicago, 1970), 16.

⁴³ Hans Rosenberg, “The Rise of the Junkers in Brandenburg-Prussia, 1410–1653, Part II,” *AHR*, 49 (January 1944): 239–41; Craig, *Politics of the Prussian Army*, 3–4; Anderson, *Lineages of the Absolutist State*, 240–41, 269–71; Robert M. Berdahl, *The Politics of the Prussian Nobility: The Development of a Conservative Ideology, 1770–1848* (Princeton, N.J., 1988), 107–57; Blum, *End of the Old Order*, 387; J. H. Clapham, *Economic Development of France and Germany, 1815–1914* (Cambridge, 1968), 41–46; Theodore S. Hamerow, *Restoration, Revolution, and Reaction: Economics and Politics in Germany, 1815–1871* (Princeton, 1958), 45–54. For peasants with favorable tenures, indemnification could be accomplished by surrendering a portion of property to the nobleman, thus bolstering the holdings of the latter even as his claims on the former diminished.

⁴⁴ Hamerow, *Restoration, Revolution, and Reaction*, 221–25; Berdahl, *Politics of the Prussian Nobility*, 264–310; Tom Kemp, *Industrialization in Nineteenth Century Europe* (London, 1969), 88–90; Alexander Gerschenkron, *Bread and Democracy in Germany* (New York, 1966), 23–24; J. A. Perkins, “The Agricultural Revolution in Germany, 1850–1914,” *Journal of European Economic History*, 10 (Spring 1981): 96–97, 101–08; Alan Richards, “The Political Economy of *Gutswirtschaft*: A Comparative Analysis of East Elbian Germany, Egypt, and Chile,” *Comparative Studies in Society and History*, 21 (October 1979): 510–12.

If the Revolution of 1848 brought about final emancipation, its ultimate defeat also had important ramifications for the construction of German nationhood. To say this is not to embrace the logic of the German *Sonderweg*, which, with its essentialist assumptions concerning class and the modernization process, has of late been forcefully criticized by Geoff Eley and David Blackbourn: the notion that a failed bourgeois revolution (exemplified by parliamentary liberalism) in the nineteenth century paved the way for fascism in the twentieth. The abolition of serfdom and the formation of the Customs Union (*Zollverein*) earlier in the nineteenth century helped erect a framework for an emerging bourgeois society, at least in its legal and economic aspects. Quietly, the bourgeoisie increasingly secured conditions for the protection and reproduction of capitalist social relations and property rights, integration of the economy, the codification of laws, and a favorable environment for associational activity, whatever marks of illiberalism were left in the structure and workings of parliamentary politics. In any event, the bourgeoisie in Germany or elsewhere had never been more than a reluctant champion of political democracy.⁴⁵

The aftermath of 1848 did, however, affect the distribution of power by class in the emerging German nation-state. For, while the economic prosperity of the 1850s sped the advance of industrialism and gave the liberal struggles renewed vigor—prolonging the constitutional conflict—it also increased the fortunes of estate owners completing the transition to capitalist agriculture. In the whirl of political crisis, the military victories of the Prussian army against Austria and France in 1866 and 1870–1871 made unification possible under Prussian hegemony, Bismarck's direction, and continued Junker prominence. Conservatism prevailed in the bureaucracy; the Junkers commanded both chambers of the Prussian diet; and Prussia held dominion in the Reich through the executive, the federal council (*Bundesrat*), and the army officer corps. As for the financial and industrial bourgeoisie, they made an easy peace with the new order: more than a few wealthy bourgeois rushed to buy estates and ape the habits of the landed elite.⁴⁶

Under Bismarck's auspices, therefore, the German Reich modernized without toppling the monarchical regime. Although the unification settlement left considerable powers to the states (of no small benefit to the Junkers), the federal

⁴⁵ The case for German exceptionalism is often held up against the "authentic" bourgeois revolutions in Britain and France. But, as Eley and Blackbourn point out, British and French historians have largely abandoned the rather simplistic and schematic notions of bourgeois revolution that many German historians still assume; and they wonder whether it is possible to speak of a bourgeoisie anywhere that seized power and recast the state and politics according to its own designs. They question, in particular, the equation of developed bourgeois classes with liberal parliamentary politics and the belief that modern industrial societies worthy of the name must pass through historical stages comparable to Britain and France. Instead, Eley and Blackbourn draw attention to the legal, economic, and organizational features of capitalist development and suggest that Germany did indeed experience a bourgeois "revolution from above" over the course of the nineteenth century, especially in the 1860s and 1870s. See David Blackbourn and Geoff Eley, *The Peculiarities of German History: Bourgeois Society and Politics in Nineteenth Century Germany* (New York, 1984); Geoff Eley, *From Unification to Nazism: Reinterpreting the German Past* (Boston, 1986), 61–84 and *passim*. See also Gareth Stedman Jones, "Society and Politics at the Beginning of the World Economy," *Cambridge Journal of Economics*, 1 (March 1977): 84. For a concise and sophisticated statement on the English case, see Christopher Hill, "A Bourgeois Revolution?" in *The Collected Essays of Christopher Hill*, 3 vols. (Amherst, Mass., 1986), 3: 94–124.

⁴⁶ Sheehan, "Conflict and Cohesion," 70–72, 77–79; Gordon A. Craig, *Germany, 1866–1945* (New York, 1980), 39–43; Fritz Stern, "Prussia," in Spring, ed., *European Landed Elites*, 60–61; Gillis, "Aristocracy and Bureaucracy," 113–20. Even if we grant the bourgeoisie a far more dynamic and important role in the nineteenth century than do the proponents of the German *Sonderweg*, the Junkers' power and influence in the Second Empire cannot be doubted. For some thoughtful comments, see Richard J. Evans, "The Myth of the Missing Revolution: Capitalism and Feudalism in Modern German History," *New Left Review*, 149 (January–February 1985): 67–94.

government held authority over commercial and tariff policy, banking, coinage, international exchange, and weights and measures. Contracts for armaments benefited the coal and iron producers of the Ruhr valley.⁴⁷ And, when economic depression in the 1870s brought cries for protection, notably from the iron and textile manufacturers, the free-trader Bismarck saw political opportunities beckoning and turned a sympathetic ear. A tariff offered him not only a means to augment the financial moorings of the state but also an occasion to harass the liberal parties and smash the left, which together had a commanding position in the national assembly (*Reichstag*). Imposing a tariff was not an easy task, for the Junkers had been staunch foes of such protectionism and joined with liberals and commercial interests to lower trade barriers. Yet the combination of international competition and falling grain prices posed serious threats to the Junkers, too, and created an opening. The establishment of duties on imported grain promised to lure the Junkers away from economic liberalism, obtain their support for industrial protection, and cement an already developing conservative coalition of big agriculture and industry. By the late 1870s, the main parties had reached agreement. Two unsuccessful attempts on the emperor's life then turned the tide against the liberals in the *Reichstag* and enabled Bismarck to pass an anti-socialist law in 1878. The next year saw the tariff of 1879 and the famed "marriage of iron and rye."⁴⁸

It was a "marriage," nonetheless, that, in the words of Gordon Craig, "assured the great landowners . . . of all but a dominant position." In the years that followed, Prussia's role in the Reich swelled, while the Junkers, displaying political flexibility, fortified their advantages. Tariffs were raised on two occasions between 1879 and 1887; and, when confronted by the Caprivi government's anti-protectionist policies in the early 1890s, the estate owners were able to use the newly founded and mass-based Agrarian League to good effect. A protected domestic market uniting rural proprietors large and small received added boosts in return for agrarian support for a naval build-up and acceptance of *Sammlungspolitik*—the assault against the Social Democrats. For their part, the Junkers eventually aimed at an imperialist "drive to the east." The results were expansionist tendencies abroad and mounting repression at home.⁴⁹ Having weathered the challenges of economic instability, emancipation, and parliamentary liberalism, the Junkers accepted a conservative unification of Germany and maintained their power in the Reich. Using the apparatus of the state with the aid of Bismarck, they preserved their economic status and figured significantly in the political economy and culture of the

⁴⁷ Craig, *Germany*, 39–41; Sheehan, "Conflict and Cohesion," 81.

⁴⁸ Gerschenkron, *Bread and Democracy*, 42–44; Peter Gourevitch, *Politics in Hard Times: Comparative Responses to International Economic Crises* (Ithaca, N.Y., 1986), 83–103; Craig, *Germany*, 63–64, 85–97.

⁴⁹ Hans-Jurgen Puhle, "Lords and Peasants in the Kaiserreich," in Moeller, ed., *Lords and Peasants in Modern Germany*, 81–109; Shelley Baranowski, "Continuity and Contingency: Agrarian Elites, Conservative Institutions, and East Elbia in Modern German History," *Social History*, 12 (October 1987): 303–04; Craig, *Germany*, 98–99, 144–67; Stern, "Prussia," 59–65; Kemp, *Industrialization in Europe*, 104–05; Barkin, *Controversy over German Industrialization*, 37–40. The close and longstanding ties between the Junkers and the Prussian army, together with the growing productive capacity of Germany industry, fueled an expansionist foreign policy, even though the Junkers were far more interested in the lands in Eastern and east-central Europe than they were in colonial ventures overseas. On the domestic front, a paternalist social welfare program and iron-fisted anti-socialism were the two sides of Bismarck's policy coin. Regarding peasant support for agricultural protectionism, and thus for the Junkers, see Robert G. Moeller, "Peasants and Tariffs in the Kaiserreich: How Backward Were the 'Bauern'?" *Agricultural History*, 55 (October 1981): 370–84. Germany was hardly the lone, or even the most prominent, expansionist power of that era.

nation for several disastrous decades to come. It would take two wars in the twentieth century to seal their fate.⁵⁰

VIEWS FROM THE PERSPECTIVE OF GERMANY AND BRAZIL, and for that matter of most Western nations during the nineteenth century, the American experience of unification is strikingly different. Much currency has been accorded the notion that the South and its landed elite "lost the war but won the peace."⁵¹ And, insofar as the notion speaks to the limits of federal reconstruction—defined in terms of its most far-reaching possibilities—and to the conservative drift of the Republican party after 1867, particularly the attempts to align with whiggish Southern whites at the expense of freedmen and small producers generally, it is compelling. Although some interpretive controversy surrounds the events that led directly to the Republican acceptance of "home rule" for the South in 1877, it surely stands as a signal event in American political history.⁵² More remarkable from a comparative vantage point is the fact that a coalition (a "solidarity bloc," as Alexander Gerschenkron calls it) of big agriculture in the South and big industry in the North did not emerge. The political bloc coming to dominate the new nation-state scarcely included the Southern landed elite.

For some time now, economists and economic historians have been questioning what was once a conventional wisdom: that the Civil War played a central role in speeding the course of American industrialization.⁵³ The issues remain in dispute. But there can be little doubt that the war decisively affected the *political economy* of industrialization. With most of the Southern states out of the Union, the Republican party enacted banking, monetary, tariff, land, and tax policies that established the sovereignty of the federal government, helped channel capital to heavy industry at the expense of other economic sectors, and greatly enlarged the domestic market for industrial goods. In light of the longstanding opposition of the South to such federal initiatives, which set the boundaries of national economic policy before 1861, the shift in power and authority was all the more dramatic. Even Robert Sharkey, who effectively demolished the portrait of a united business-

⁵⁰ Arno Mayer, *The Persistence of the Old Regime: Europe to the Great War* (New York, 1981), 329; Gerschenkron, *Bread and Democracy*, x–xiii and *passim*; Baranowski, "Continuity and Contingency," 285–308; Hans Rosenberg, "The Pseudo-Democratization of the Junker Class," in Georg Iggers, ed., *The Social History of Politics: Critical Perspectives in West German Historical Writing since 1945* (Heidelberg, 1985), 91–94, 103–09. This is not to argue, however, that the persisting power and influence of the Junkers explains or leads logically to the rise of fascism.

⁵¹ See, for example, Michael Perman, *Reunion without Compromise: The South and Reconstruction, 1865–1868* (Cambridge, 1973); William Gillette, *Retreat from Reconstruction, 1869–1879* (Baton Rouge, La., 1980); Elizabeth Studley Nathans, *Losing the Peace: Georgia Republicans and Reconstruction, 1865–1871* (Baton Rouge, 1968); Leon F. Litwack, *Been in the Storm So Long: The Aftermath of Slavery* (New York, 1979); Woodward, "Price of Freedom," 113.

⁵² The controversy involves the contested presidential election of 1876 and the ensuing "compromise of 1877." See C. Vann Woodward, *Reunion and Reaction: The Compromise of 1877 and the End of Reconstruction* (Boston, 1951); Allan Peskin, "Was There a Compromise of 1877?" *Journal of Southern History*, 60 (June 1973): 63–75; Michael Les Benedict, "Southern Democrats in the Crisis of 1876–77: A Reconsideration of *Reunion and Reaction*," *Journal of Southern History*, 46 (November 1980): 489–524; C. Vann Woodward, "Yes, There Was a Compromise of 1877," *Journal of American History*, 60 (June 1973): 215–23.

⁵³ See, for example, Thomas C. Cochran, "Did the Civil War Retard Industrialization?" *Mississippi Valley Historical Review*, 48 (September 1961): 191–210; Stanley L. Engerman, "The Economic Impact of the Civil War," *Explorations in Entrepreneurial History*, 3 (Spring–Summer 1966): 176–99; and the essays by George Rogers Taylor and Alfred D. Chandler in David T. Gilchrist and W. David Lewis, eds., *Economic Change in the Civil War Era* (Greenville, Del., 1965), 1–22, 137–51.

Republican front, has insisted that wartime legislation clearly "tipp[ed] the class and sectional balance" in favor of the East over both the South and West. This, he claimed, was "the momentous change" between 1850 and 1873: a change that enabled large industrialists and bankers to consolidate and reorganize after the ensuing depression.⁵⁴

The Republican hold on the national polity was not, however, firmly secured by the war and Reconstruction. Party competition intensified during the 1870s and, together with widespread social and economic dislocations, threatened Republican control over the state. In 1874, Democrats won a majority in the House of Representatives for the first time since 1858; a revitalized labor movement pressed for the eight-hour day and contributed to the appearance of new political parties and independent tickets at the local, state, and national levels; a severe depression produced massive unemployment and mounting social strife; and in 1877 the most destructive labor conflict of the nineteenth century engulfed railroads from the Middle Atlantic through the Midwest, bringing the intervention of federal troops.⁵⁵ Having established a national framework of market relations and having largely purged its radical wing in the North and South, the Republicans might then have looked to the "classic conservative coalition" to maintain their rule.

Despite flirtations in that direction, no American analogue to the German "marriage of iron and rye," linking the Rhineland and East Prussia, or to the Brazilian alliance of *café com leite*, linking São Paulo and Minas Gerais, ever emerged. Republicans offered limited encouragement and few resources, even to sympathetic Southerners during the postwar period. And the party had enough influential leaders who "waved the bloody shirt" and kept "sectional issues" alive to circumscribe further overtures southward while fanning the hostility of cotton planters and their supporters. Federal subsidies for internal improvements did figure importantly in the Republican strategy to attract whiggish Southern Democrats and build a stable political base in the South, but the events of 1877, when assistance to the Texas and Pacific Railroad failed to materialize, showed the strategy to be problematic under what seemed the best of circumstances. Subsequent Republican administrations and presidential aspirants vacillated in their "southern policy." For the most part, they sought to build up their protectionist, sound-money bloc through tariff appeals to industrialists and to sugar, rice, and tobacco planters on the Southern periphery rather than through compromise with the cotton interest in the Southern heartland. Even these efforts came to little.⁵⁶

⁵⁴ Robert P. Sharkey, *Money, Class, and Party: An Economic Study of the Civil War and Reconstruction* (Baltimore, Md., 1959); Robert P. Sharkey, "Commercial Banking," in Gilchrist and Lewis, *Economic Change in the Civil War Era*, 27. We still need to learn more about how interest rates, taxation, and investment patterns—all shaped by federal policy—affected income distribution and capital flows both regionally and within economic sectors. But research on developing capital markets in the postwar period does point to the National Banking Acts, passed during the war, as a significant factor in explaining the high interest rates and other financial disadvantages that beset the South. See Lance E. Davis, "The Investment Market, 1870–1914: The Evolution of a National Market," *Journal of Economic History*, 25 (September 1965): 356–92; John A. James, "Financial Underdevelopment in the Postbellum South," *Journal of Interdisciplinary History*, 11 (Winter 1981): 443–53. For some excellent observations and questions, see Harry N. Scheiber, "Economic Change in the Civil War Era: An Analysis of Recent Studies," *Civil War History*, 11 (December 1965): 396–411.

⁵⁵ David Montgomery, *Beyond Equality: Labor and the Radical Republicans, 1862–1872* (New York, 1967); Herbert G. Gutman, *Power and Culture: Essays on the American Working Class*, ed. Ira Berlin (New York, 1987), 70–92, 117–212; Paul Kleppner, *The Third Electoral System, 1853–1892: Parties, Voters, and Political Cultures* (Chapel Hill, N.C., 1979), 257–81; John M. Dobson, *Politics in the Gilded Age: A New Perspective* (New York, 1972).

⁵⁶ Richard H. Abbott, *The Republican Party and the South, 1855–1877* (Chapel Hill, N.C., 1986), 233–44; Terry L. Seip, *The South Returns to Congress: Men, Economic Measures, and Intersectional*

Propelled by what Richard Franklin Bensel has termed the “military pension-tariff engine of national development,” the Republicans forged a coalition that included substantial sections of the skilled industrial working class, commercial farmers relying on the domestic market, a great many Civil War veterans, and pietistic Protestants, who tended to be found among the other groups or the urban middle class. But the party’s policy-making core was composed of heavy industrialists, national bankers, and textile manufacturers. The South, particularly the cotton-growing landed elite, remained within the Democratic low-tariff coalition, which included financiers and exporters on both coasts, as well as farmers looking chiefly to overseas markets and ethnic groups in urban areas—an alliance that mixed internationalist and localist concerns.⁵⁷ While the Democrats obtained popular pluralities in all save one presidential election from 1876 to 1892 (although on only two occasions won the presidency) and normally commanded the House with the Southern faction looming largest, almost every major policy conflict was resolved in favor of big industry. Even when the South and West joined forces on key issues like the money supply, industrial interests usually effected a suitable compromise. Thus the Bland-Allison Act of 1878 not only avoided the unlimited coinage of silver (which Southern farmers and planters generally wanted) but also received the firm support of the congressional delegation from iron-producing Pennsylvania. During the 1890s, when economic depression and the merger movement permitted the triumph of the agrarian free-silver wing within the Democratic party, once-loyal investment and commercial bankers jumped to the Republicans and completed the political bloc that would govern the “System of 1896.” As one scholar of comparative politics remarked, “In no other country did [industrialists] share power so little.”⁵⁸

The failure of the “classic conservative coalition” to emerge, and, therefore, of the southern landed elite to retain significant influence over national life, is further evidenced by the region’s relative exclusion from a number of strategic positions that had been virtual Southern preserves before the Civil War. While Southerners

Relationships, 1868–1879 (Baton Rouge, La., 1983), 87–99; Woodward, *Reunion and Reaction*; Vincent P. DeSantis, *Republicans Face the Southern Question: The New Departure Years, 1877–1897* (Baltimore, Md., 1959), 36, 66–103; Stanley P. Hirshon, *Farewell to the Bloody Shirt: Northern Republicans and the Southern Negro, 1877–1893* (Chicago, 1968), 123–89. The threat of federal control of national elections hung over the South until the 1890s, and thereby kept the Southern landed elite on the defensive. Although the Lodge Bill failed, there was almost constant federal involvement in contested Southern elections.

⁵⁷ Richard Franklin Bensel, *Sectionalism and American Political Development, 1880–1980* (Madison, Wis., 1984), 60–103; Keller, *Affairs of State*, 195–96, 238–50; Tom E. Terrill, *Tariffs, Politics, and American Foreign Policy* (Westport, Conn., 1973), 10–33; Paul Kleppner, *The Cross of Culture: A Social Analysis of Midwestern Politics, 1850–1900* (New York, 1970); Lewis L. Gould, “The Republicans Search for a National Majority,” in H. Wayne Morgan, ed., *The Gilded Age* (Syracuse, N.Y., 1970), 171–87; R. Hal Williams, “Dry Bones and Dead Language: The Democratic Party,” in *ibid.*, 129–48. Most recent studies of party politics in the nineteenth century focus on state and local voting patterns, deemphasize the significance of national issues, and underscore the importance of ethno-cultural factors in electoral behavior. Little effort has been devoted to linking voting behavior with elite coalitions and policy formation. I am not denying that local and cultural issues were key aspects of partisan identification; rather, I am pointing to connections between partisan identification and questions of political economy, which were central in establishing the context for local issues and divisions.

⁵⁸ Peter A. Gourevitch, “International Trade, Domestic Coalitions, and Liberty: Comparative Responses to the Crisis of 1873–1896,” *Journal of Interdisciplinary History*, 8 (Autumn 1977): 299–305; Walter Dean Burnham, *Critical Elections and the Mainsprings of American Politics* (New York, 1970), 34–90; Thomas Ferguson, “From Normalcy to New Deal: Industrial Structure, Party Competition, and American Public Policy in the Great Depression,” *International Organization*, 38 (Winter 1984): 41–94; Allen Weinstein, *Prelude to Populism: Origins of the Silver Issue, 1867–1878* (New Haven, Conn., 1970), 331–53; Carl V. Harris, “Right Fork or Left Fork? The Sectional Party Alignments of Southern Democrats in Congress, 1873–1897,” *Journal of Southern History*, 42 (November 1976): 471–506.

had dominated the presidency, the Supreme Court, the speakership of the House of Representatives, and the diplomatic corps during the antebellum period, they claimed but a handful of such posts thereafter: between 1865 and 1912, only 7 of 31 Supreme Court appointments, 2 of 12 House speakerships, 14 of 133 cabinet positions, and, excepting the rather unusual case of Andrew Johnson, not even one presidential or vice presidential nomination, let alone an office. "Never in the history of the country, and rarely in the history of any country," C. Vann Woodward wrote, "had there been a comparable shift in the geography of political power."⁵⁹ The presence of landed aristocrats in European and Latin American governing circles into the twentieth century (well into the century, in some instances) stands as a stark contrast. So, too, does what has variously been called the "gentrification," "nobilization," or "feudalization" of the bourgeoisie in the wake of other Western unification struggles. The cult of the "Lost Cause" and the romanticization of plantation life had more than sectional appeal in postwar America, as did a racism etched into national law by the end of the nineteenth century. An antimodernism that occasionally celebrated the Middle Ages and the pastoral ideal also found an audience amid the teeming cities of the industrial North. But antimodernism, as T. J. Jackson Lears has demonstrated, ultimately contributed to the remaking and reinforcing of bourgeois values and class hegemony. Except for the brief Northern planter movement of the middle to late 1860s, there was no important American counterpart to the bourgeois acquisition of landed estates and noble titles.⁶⁰

The dramatic setbacks suffered by the South and its planters on the national front did not bring much in the way of compensation regionally. Indeed, the national setbacks both promoted and accentuated Southern woes of the postwar decades. This is not to discount the internal factors that made for poverty and limited development in the post-bellum South: the physical repercussions of the war, the traditionalism of many large landowners, the exploitative practices of merchants and landlords, and a rampant racism, to name but a few.⁶¹ Nor is it to paint a portrait of general stagnation, for the postwar period saw the refashioning of social relations in the plantation districts, the integration of the white yeomanry into the staple market, the rapid growth of towns and cities, and the appearance of new industrial enterprises in rural and urban areas alike.⁶² It is not to suggest that the North simply plundered the South. Rather, it is that the fate of the Southern economy cannot be understood apart from the shape of the national political economy.

⁵⁹ Woodward, *Origins of the New South*, 456–57.

⁶⁰ Lawrence Powell, *New Masters: Northern Planters during the Civil War and Reconstruction* (New Haven, Conn., 1980); T. J. Jackson Lears, *No Place of Grace: Antimodernism and the Transformation of American Culture, 1880–1920* (New York, 1981); Woodward, *Origins of New South*, 154–57; Gaines M. Foster, *Ghosts of the Confederacy: Defeat, the Lost Cause, and the Emergence of the New South* (New York, 1987); Mayer, *Persistence of the Old Regime*, 19, 129–87. Northerners who went South to buy or lease plantations during the war and early phases of Reconstruction usually failed miserably, but, as Powell pointed out, they generally aspired to the status of absentee landlords rather than that of landed gentry. In Europe, according to Arno Mayer, landed property was the principal form of personal wealth, even in England, as late as 1914.

⁶¹ See, for example, Roger L. Ransom and Richard Sutch, *One Kind of Freedom: The Economic Consequences of Emancipation* (Cambridge, 1977); Wiener, *Social Origins of the New South*, 3–133, 186–221; Billings, *Planters and the Making of a "New South"*, 25–131.

⁶² See Harold D. Woodman, "The Reconstruction of the Cotton Plantation in the New South," in Glymph and Kushma, eds., *Essays on the Postbellum Southern Economy*, 95–119; Wayne, *Reshaping of Plantation Society*, 110–96; Hahn, *Roots of Southern Populism*, 137–203; Wright, *Old South, New South*, 17–155; David L. Carlton, *Mill and Town in South Carolina, 1880–1920* (Baton Rouge, La., 1982).

Planter resistance to industrialization in the South was not inconsiderable. It had a long history and a sound logic. Before the Civil War, cities and industries were often viewed as antithetical to the stability of the slave regime; after the war, industrial development was commonly seen as a threat to planter control of the free black labor force. Not by accident did Southern textile manufacturers depend chiefly on white operatives or railroad and mining companies look to the leasing of convicts. But it is easy to exaggerate the planters' hostility to modernization and their conflicts with industrialists. More than a few wealthy landowners and their kin adjusted to the postwar world by seeking innovations in agriculture and by investing in merchandising, mills, mines, lumbering, and railroads.⁶³ Their prospects, however, continued to rest heavily on the health of a cotton economy that came to dominate more and more of the region; and the cotton economy entered a state of gradual and, save for some brief upturns, irreversible deterioration. Thus, although figures for the post-bellum South as a whole show relatively rapid recovery in output and, after 1880, rates of economic growth similar to the North, the cotton states experienced both sharper declines following the war and slower resuscitation.⁶⁴

The direct effects of the Civil War, coupled with longer term unfavorable conditions in the international market, including a leveling off in the demand for cotton, accounted for many of the region's economic problems. But the fall of the South from national power and the contours of the new political economy made them all the more difficult to overcome. Protectionism forced cotton growers to sell cheap and buy dear; the national banking system institutionalized a scarcity of regional capital and produced high interest rates; and the structure of postwar revenue collection, before the implementation of income and corporate taxes, placed fiscal burdens most heavily on consumers and landowners. Together, these policies funneled surplus income out of the South, hampered investment, and helped put the region in a quasi "colonial" status in relation to the North by the end of the nineteenth century. The lack of adequate capital stymied the transition to wage labor and a more general reorganization of production in the countryside, thereby contributing to the spread of tenancy and sharecropping, and it decidedly limited the options of aspiring manufacturers and entrepreneurs, who were left to rely on a local, highly personal, and shallow financial market. In 1880, per capita bank deposits in the South represented only one-fifth the figure for the entire nation, and, as late as the first decade of the twentieth century, interest rates were systematically higher than in any other region.⁶⁵ Southern planters charged, moreover, that federal economic policies, especially high tariffs, amounted to sectional taxes that promised to usurp their property. But there was no significant

⁶³ Cobb, "Beyond Planters and Industrialists," 46–56; John J. Beck, "Building the New South: A Revolution from Above in a Piedmont County," *Journal of Southern History*, 53 (August 1987): 446–70; Edward L. Ayers, *Vengeance and Justice: Crime and Punishment in the Nineteenth-Century South* (New York, 1984), 185–222; Carlton, *Mill and Town in South Carolina*, 42–50; Billings, *Planters and the Making of a "New South,"* 42–95. On the threats posed by urbanization and industrialization, see Eugene D. Genovese, *The Political Economy of Slavery: Studies in the Economy and Society of the Slave South* (New York, 1965), 180–220; Barbara Jeanne Fields, *Slavery and Freedom on the Middle Ground: Maryland during the Nineteenth Century* (New Haven, Conn., 1985), 40–62; Wiener, *Social Origins of the New South*, 137–221.

⁶⁴ Engerman, "Economic Adjustments to Emancipation," 208–09; Wright, *Political Economy of the Cotton South*, 158–84.

⁶⁵ Gerald David Jaynes, *Branches without Roots: The Genesis of the Black Working Class in the American South, 1862–1882* (New York, 1986), 147–57; David L. Carlton and Peter A. Coclanis, "Capital Mobilization and Southern Industry, 1880–1920: The Case of the Carolina Piedmont," *Journal of Economic History*, 49 (March 1989): 73–94; James, "Financial Underdevelopment in the Postbellum South," 43–56.

relief forthcoming. Southern Democrats pressed for monetary inflation, repeal of the 10 percent tax on state bank notes, reduction of the tariff, and an income tax—and lost. As the majority faction in the Democratic party, the South did make its presence felt in Congress; yet, until the twentieth century was well advanced, only home rule could be secured.⁶⁶

Considering the devices Prussian Junkers and Brazilian *fazendeiros* used to shore up their agricultural enterprises in the midst of similarly intense international market pressures, it is worth asking why Southern cotton planters did not at least push for similar solutions. Part of the answer is that Southerners depended far more on income from exports than did their Prussian counterparts, while they did not face serious foreign competition in the domestic market. Protectionist trade-offs, therefore, had little to offer.⁶⁷ The Brazilian route of valorization might have seemed more promising. But, here, the weakness and vulnerability of the postwar planters again becomes apparent. It was not that such ideas eluded cotton growers: from the 1870s, in state and local agricultural societies as well as in the elite-dominated Grange, they experimented with a variety of schemes designed to boost commodity prices and lower purchasing costs. Instead, the planters found that they lacked not only the power nationally to enlist the necessary federal assistance but also a firm enough political base regionally to move forward in their own interest. Challenges to their hegemony came from an assortment of “modernizers” who hoped to diversify the Southern economy and, especially, from white family farmers who, having been drawn into the cotton market, suffered from spiraling indebtedness and entertained more radical political ideas, including the possibility of a biracial coalition of small producers. The subtreasury plan, which proposed a federally funded system of crop marketing and low interest credit, thus served to divide the Southern Farmers’ Alliance in the early 1890s and prepare the way for Populism. Only when the People’s party was defeated and the disfranchisement of poor blacks and whites completed did planters seek to enact what are often deemed “populist” programs—programs that favored large landowners when they came to fruition in the 1930s. Neither the Prussian Junkers nor the Brazilian *fazendeiros* encountered comparable threats from below.⁶⁸

⁶⁶ Harris, “Right Fork or Left Fork?” 484–96, 501–03; Potter, *South and the Concurrent Majority*, 43–56.

⁶⁷ Sugar planters in Louisiana were in a different position. They did face foreign competition and did look to tariff protection. Brazilian coffee planters, who were even more dependent on the export market, supported a protective tariff—a stance that testified to their power in and identification with the state and to the dominance of agriculture over industry. As Warren Dean pointed out, the First Republic spent more money than did the empire and thus needed a reliable and substantial source of revenue. A tariff, from the planter’s perspective, was a desirable alternative to taxes on land, income, or profits. See Dean, *Industrialization of São Paulo*, 70–71; John Alfred Heitmann, *The Modernization of the Louisiana Sugar Industry, 1830–1910* (Baton Rouge, La., 1987), 90–95. Southern opposition to the tariff and support for an income tax reflected the subordinate position of agriculture in the American economy and state.

⁶⁸ Theodore Saloutos, *Farmer Movements in the South, 1865–1933* (Berkeley, Calif., 1960), 36–39, 153–281; Solon J. Buck, *The Granger Movement* (Cambridge, Mass., 1913), 252–55, 264–66; William Warren Rogers, *The One-Gallused Rebellion: Agrarianism in Alabama, 1865–1896* (Baton Rouge, La., 1970), 68–73; Lawrence Goodwyn, *Democratic Promise: The Populist Moment in America* (New York, 1976), 154–243, 515–55; George B. Tindall, *The Emergence of the New South, 1913–1945* (Baton Rouge, 1967), 111–42, 391–432; Daniel, *Breaking the Land*, 65–183. Southerners had long been wary of enlarging the powers of the federal government, and that wariness certainly affected their behavior on this question. But the wariness cannot be separated from their weakened position in the nation-state and in the region. After all, during the 1850s, slaveholders demanded federal protection for slavery in the territories, and in the 1930s they welcomed the Agricultural Adjustment Act. For an insightful though somewhat different point of view, see Harry N. Scheiber, “Federalism, the Southern Regional Economy, and Public Policy since 1865,” in David J. Bodenhamer and James W. Eley, eds., *Ambivalent Legacy: A Legal History of the South* (Jackson, Miss., 1984), 69–105. On the absence of a serious threat from below in

There was nothing inevitable about the outcome of these political developments. The war for the Union could well have failed much as did the revolutions of 1848 in Europe, postponing unification and strengthening the Southern landed elite when unification ultimately occurred (presuming that it would occur). However much historians tend to assess the impact of the Civil War and Reconstruction in terms of the most progressive possibilities and however tempting it may be to speculate about the repercussions of a more "radical" Reconstruction, the likely alternatives within the national and international context of the mid-nineteenth century were all in the other direction. Indeed, as Barrington Moore, Jr., has suggested, we might well ponder what anything short of a total Union victory would have meant for American society.⁶⁹

That the Southern landed elite managed to remain a dominant force within the South depended on a cross-sectional community interest of property and race created by cotton, mounting labor unrest nationwide, and the intellectual and political currents of a new age of imperialism. Few Northerners imagined that the postwar South would not continue to be a cotton-producing region, and more and more came to believe that white planters were best suited to run it.⁷⁰ The granting of "home rule" in part confirmed this. But if home rule enabled the big landowners to recapture a significant measure of regional power, it did not enable them to recapture substantial national power. And their failure on the national level contributed to the erosion of their regional base. In comparative perspective, what stands out in the course of emancipation and unification is the swift and dramatic decline in the fortunes of the Southern planter class.

Prussia and Brazil, see Puhle, "Lords and Peasants in Kaiserreich," 98–100; Topik, *Political Economy of the Brazilian State*, 8–11.

⁶⁹ Moore, *Social Origins of Dictatorship and Democracy*, 149–55.

⁷⁰ Eric Foner, *Politics and Ideology in the Age of the Civil War* (New York, 1980), 108–09; Powell, *New Masters*, 97–155; Montgomery, *Beyond Equality*; Du Bois, *Black Reconstruction*; E. J. Hobsbawm, *The Age of Empire, 1875–1914* (New York, 1987), 56–83.

Race and Class in the South African Countryside: Cultural Osmosis and Social Relations in the Sharecropping Economy of the South-Western Transvaal, 1900–1950

CHARLES VAN ONSELEN

AS RECENTLY AS 1979, A SCHOLAR REFLECTING ON SOUTH AFRICAN HISTORIOGRAPHY was moved to comment that, "in contrast with the situation abroad, the study of agrarian history in South Africa is still in its infancy."¹ While undoubtedly an accurate summation of the somewhat bleak position prevailing at the time, during the years that followed, two influential articles achieved the status of seminal works, albeit in different traditions. The first of these, an essentially empirical exploration of the emergence and decline of the South African peasantry, charted the alacrity with which black farmers had responded to the creation of new markets for agricultural products after the mineral discoveries of the late nineteenth century and then went on to outline the demise of their structural importance in the economy during the period that followed.² The second, a work predicated on more theoretical concerns, sought to demonstrate the development of capitalism in South African agriculture by exploring class struggles in the countryside during the early twentieth century.³

A decade later, these articles have—in a slightly misleading and somewhat contrived fashion, perhaps—come to epitomize the polar extremities of a vigorous and sometimes acrimonious exchange between social historians and structuralists about the nature and importance of the rural transformation that accompanied the South African industrial revolution.⁴ Despite the excessive heat generated in the course of this debate, there have also been sufficient real advances made in the study of agrarian history for the field no longer to be characterized as being in its infancy. Indeed, the rapidly growing number of works devoted to an examination of the commercialization and capitalization of white agriculture, the changing class structures of the countryside, and the challenges mounted by the rural working

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¹ C. Eloff, quoted by C. Bundy, "Assessing the Harvest: Some Perspectives on South Africa's Rural History in the 19th and 20th Centuries," paper delivered at the National Conference of the South African Historical Society, Cape Town, January 1985.

² C. Bundy, "The Emergence and Decline of a South African Peasantry," *African Affairs*, 71 (1972): 369–88.

³ M. Morris, "The Development of Capitalism in South African Agriculture: Class Struggle in the Countryside," *Economy and Society*, 5 (1976): 292–343.

⁴ The major issues in this debate are critically examined in H. Bradford, "Highways, Byways and Cul-de-sacs: The Transition to Agrarian Capitalism in Revisionist South African Historiography," *Radical History Review*, 47–48 (Winter 1989), forthcoming.

classes have provided valuable new insights into the circumstances of black labor tenants, sharecroppers, and wage laborers during the twentieth century.⁵

For all these advances, however, it will be some time yet before these new studies can be inserted into a comparative historical framework with any accuracy. While much has been accomplished, much remains to be done. In the case of sharecropping, for example, the precise regional economic trajectories that the institution assumed in the South African context have yet to be fully charted, let alone understood. Thus, in the United States, sharecropping, although powerfully circumscribed by peonage and the continued political power of Southern landlords in the late nineteenth century, represented a step up from slavery for those blacks who set out on the unpromising road to wage labor. In South Africa, by contrast, sharecroppers were usually drawn from the ranks of an increasingly beleaguered black peasantry, and crop-sharing arrangements often heralded a step down onto the path to proletarianization. This difference is further illustrated by the fact that, in the American South, it was the advent of the mechanical cotton picker, a machine that did much to free a relatively affluent landlord from the need for black labor, that signaled the decline of sharecropping. In South Africa, it was the arrival of the gasoline-driven tractor, a machine that delivered impoverished Afrikaner farmers from their dependence on one of the instruments of production—the draft oxen belonging to their African tenants—that marked the eventual eclipse of the institution.⁶

Also, the economic structure of the post-bellum South briefly permitted and, arguably, even encouraged the emergence of a strand of agrarian populism that incorporated both black and white tenant farmers. In twentieth-century South Africa, the routes followed by upwardly mobile Afrikaner landlords who enjoyed politically privileged access to the state only intersected tangentially with those of unenfranchised black sharecroppers locked into a colonial order. Under these circumstances, racially exclusive ideologies, such as those espoused by Afrikaner nationalists during the 1914 Rebellion, by black leaders of the Industrial and Commercial Workers' Union (ICU), or by the ministers of the African Methodist Episcopal church (AMEC) in the late 1920s, often held far greater appeal for their socially distanced constituents.

But, even in economies historically dominated by sharecropping arrangements, such as that of the American South, there was no necessary congruence between ideology and social reality, between what leaders said and the people did. This lesson was not lost on Tom Watson, that wily old "Agrarian Rebel" who "spoke repeatedly from the same platform with Negro speakers to mixed audiences of Negro and white farmers": "He did not advocate 'social equality' and said so emphatically, since that was 'a thing each citizen decides for himself.' But he insisted on 'political equality,' holding that 'the accident of colour can make no difference in the interests of farmers, croppers and labourers.'"⁷ Watson's artful formulation begged a more interesting question: to what extent did the institution of sharecropping in fact facilitate the emergence of a measure of "social equality"?

⁵ Among the most important of these works are: W. Beinart, P. Delius, and S. Trapido, eds., *Putting a Plough to the Ground: Accumulation and Dispossession in Rural South Africa 1850–1930* (Johannesburg, 1986); H. Bradford, *A Taste of Freedom: The I.C.U. in Rural South Africa, 1924–1930* (New Haven, Conn., 1987); and T. J. Keegan, *Rural Transformations in Industrialising South Africa: The Southern Highveld to 1914* (Johannesburg, 1986).

⁶ See, for example, G. M. Fredrickson, *White Supremacy: A Comparative Study in American and South African History* (Oxford, 1981), 210.

⁷ C. Vann Woodward, *Tom Watson: Agrarian Rebel* (Oxford, 1979), 221.

One only has to take cognizance of the words of a sharecropper recalling his visits to the cotton gin in Tukabachee County, Alabama, at the turn of the century to be reminded that, in practice, it was often difficult to plot the line separating "political" from "social" equality. As Nate Shaw remembered it, "'They observed your turn strictly, for white and colored. It was mighty seldom that anybody would go ahead of your wagon. And while we was waiting our turns, white and colored, we'd talk about our crops and how much more we had at home and how much we done ginned and what the cotton was bringing that year.'" ⁸

Unfortunately, considerations such as these—which seek to weave together notions of race and class into complex explanatory patterns of social behavior—have yet to inform much of the new South African agrarian history. Given a paucity of detailed studies, social scientists have tended to portray race relations in the countryside as uniformly harsh and brutal. Thus, while writing what was to become the classic work of its genre in the mid-1930s, psychologist I. D. MacCrone drew attention to the many "cases of violent physical treatment which are such a feature of the relations between white and black in the country districts."⁹ A recent study probing black protest on the land in the 1920s with far more attention to African resistance still concluded that "fists, whips and guns were central in maintaining master-servant relationships on the farms."¹⁰ There is little reason to doubt the broad veracity of such accounts. Images of *sjambok*, or whip-wielding white landlords and cowering black farm workers, still occupy prominent positions in the galleries of latter-day academics, analysts, and activists. But problems arise when parts of the picture, no matter how accurate in their own right, are inserted uncritically into the larger tableau. Thus the black-white interaction on the *platteland* (the high, level plains found above the escarpment)—and the image of subordination drawn from it—has sometimes been presented as the progenitor of modern South African race relations in general and the social formation associated with capitalist apartheid in particular. In the words of one passionate observer, "The roots of apartheid are not to be found in the white cities, nor even in the endless tunnels of the gold mines of the Rand. They are buried deep in the red soil of the white owned farms, where for some 200 years, before ever South Africa became an urban industrial economy and the word apartheid was thought of, relationships were being forged between white master and black serfs."¹¹

A new generation of historians, benefiting from the steadily expanding range of studies that point to the presence of different socioeconomic "layers" in the South African countryside, has found such overarching formulations increasingly unsatisfactory. Given the distinctive social and economic positions that wage laborers, labor tenants, and sharecroppers occupied in the rural social order, it seems unlikely that white landowners or farmers would have interacted with *all* blacks on the land in a uniformly harsh or dismissive fashion. Indeed, the fact that white-black behavior on the *platteland* transcended the restrictive dynamic of master-serf interaction has long been known and appreciated in the countryside, where the politically dominant language, Afrikaans, is well endowed with words that are better able than English to capture the complex nuances of social relations on the land. Only urbanized English-speakers could gloss over the real distinctions that separate *plaas kaffers* (farm niggers) from *volk* (folk), *mense* (farm people) from

⁸ T. Rosengarten, *All God's Dangers: The Life of Nate Shaw* (New York, 1975), 196.

⁹ I. D. MacCrone, *Race Attitudes in South Africa* (London, 1937), 232.

¹⁰ Bradford, *Taste of Freedom*, 55.

¹¹ R. Ainslie, *Master and Serfs* (London, 1973), 7.

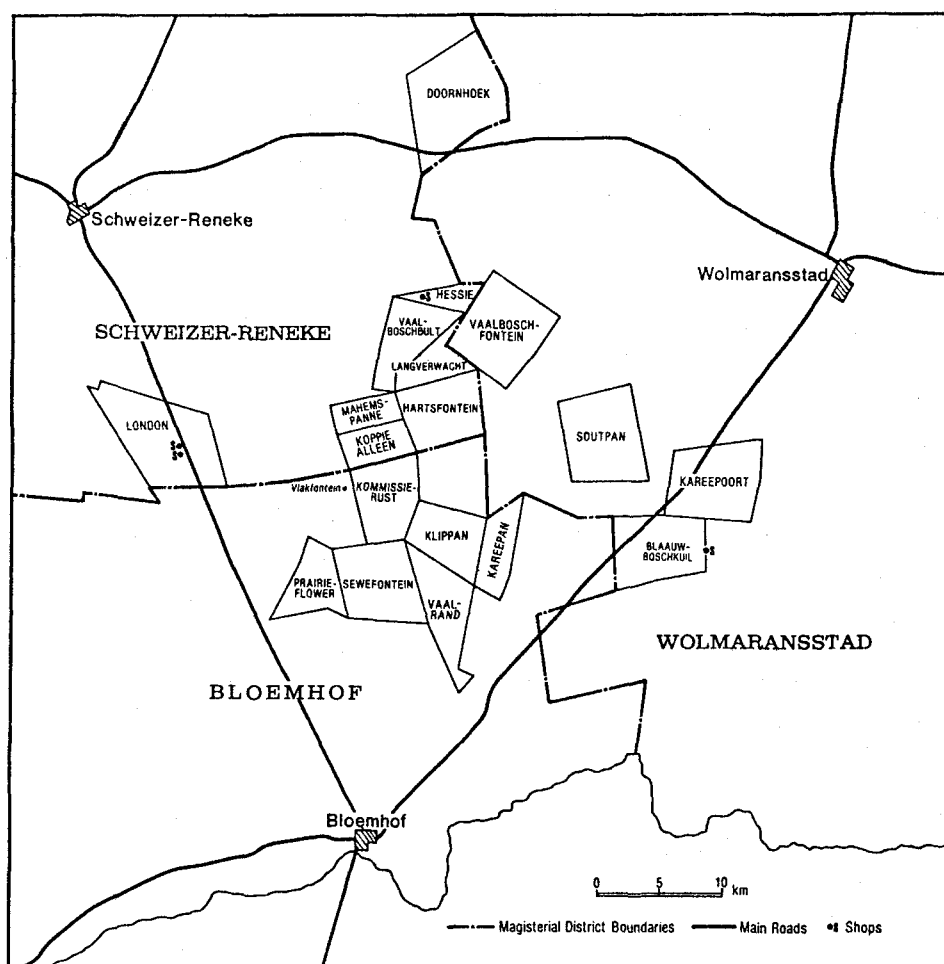
diensbodes (servants), *basters* (bastards) from *oorlams* (the capable ones), or a *kneg* (serf or bondsman) from a *voorman* (foreman). While most of these words undoubtedly have racial overtones and some are unambiguously racist, others—such as *volk* and *mense*—are more complex and have connotations of social intimacy. It is therefore incumbent on historians to determine more exactly what was the behavior of Afrikaner landlords, farmers, or *bywoners* (white tenant farmers) when they confronted African wage laborers, labor tenants, or sharecroppers on the land during much of the twentieth century.

This task is clearly of awesome proportions and exceeds the bounds of a single essay. Even with the current advances in the historiography, it will probably be several years before any social historian can provide a comprehensive account of the nature of race relations across the class spectrum in the countryside. I have chosen to probe one dimension of the problem—the extent to which a cultural synthesis and a measure of social equality developed between those white Afrikaner landlords, farmers, and *bywoners* whose economic position on the land most closely approximated that of the most privileged black tenants: the sharecroppers.¹² By concentrating on the social and economic proximity of these historical protagonists, and using evidence recorded in oral histories, I hope to demonstrate that the behavior of significant numbers of blacks and whites on the *platteland* often transcended the stark and restrictive code of race relations as it is generally understood and reached a surprising measure of accommodation in a sadly divided society. In order to do this with some precision, I will look at the South-Western Transvaal in general and then focus on the triangle created by the towns of Bloemhof, Schweizer-Reneke, and Wolmaransstad.

IN GEOGRAPHICAL TERMS, THE “CARROT-SHAPED” SOUTH-WESTERN TRANSVAAL has never been quite certain exactly where it wanted to be inserted in the subcontinent. Bounded on the north by the lower-lying Bushveld country so successfully evoked by the novelist Herman Charles Bosman, it is flanked on the west by the drylands of the northwestern Cape and the Kalahari, while, to the east, it is hemmed in by the lethargic Vaal River and the more fertile soils of the northwestern Orange Free State. To the south, the “root” points to the junction of the Vaal and Orange rivers and, beyond that, to the city of Kimberley. Unfortunately, this rather awkwardly shaped wedge is equally ambivalent about its climatic disposition. The twenty-inches-a-year isohyet (line indicating areas with equal rainfall) meanders through the point where the boundaries of the Bloemhof, Schweizer-Reneke, and Wolmaransstad magisterial districts meet. Areas to the west of this line are considered marginal for crop farming, while those to the immediate east of it are only slightly better placed. In wet years, the isohyet moves west, but in the dry years—which somehow always seem to outnumber the wet—the line moves east. In practice, this has meant that the monotonous surface of Kalahari sand, which is only occasionally broken by extruding veins of chalky limestone, has been best suited to that most perilous of all forms of agriculture: “mixed farming.”

Having been handed such an unpromising dowry by nature, the sand and gravel plains of the South-Western Transvaal have always found it difficult to attract

¹² In the case of the American South, however, some of this terrain has been charted. See, for example, J. T. Kirby, “Black and White in the Rural South, 1915–1954,” *Agricultural History*, 58 (July 1984): 411–22.



FARMS AND SHARECROPPING ACTIVITY in the Bloemhof-Schweizer-Reneke-Wolmaransstad Triangle, 1900-1950. Drawn by P. Stickler, Department of Geography, University of the Witwatersrand.

suitors interested in longer-term relationships. During much of the eighteenth and nineteenth centuries, the flood plain between the Vaal and Harts rivers was largely the preserve of bands of nomadic and semi-nomadic San and Korannas. More settled Tswana-speakers, concentrated to the south and west of the area, made use of the grassland and acacia bush largely as a hunting preserve or as supplementary grazing for their cattle.¹³ During the late nineteenth century, however, this region suddenly found itself on the fringes of two glittering, Cinderella-like occasions as, first, diamonds were discovered in the northwestern Cape in the late 1860s and then gold on the Witwatersrand in the mid-1880s. The advent of industrial capitalism and the emergence of urban markets in Kimberley and Johannesburg transformed the face of much of southern Africa, including to a large extent the South-Western Transvaal. The land between the Vaal and Harts was quickly stripped of its acacia tree covering by local entrepreneurs in order to provide badly needed fuel for the diamond fields and then—during the 1870s and 1880s—

¹³ See, for example, K. Shillington, *The Colonisation of the Southern Tswana* (Johannesburg, 1985).

invaded by hopeful burghers of the South African Republic, who carved out large farms within the Bloemhof—Schweizer-Reneke—Wolmaransstad triangle. But, when the newly demarcated tracts of land failed to reveal signs of immediate wealth in the form of precious minerals, many impatient citizens sold their farms to land speculators or the large land companies that dominated the Transvaal countryside by the turn of the century. Others stayed on and attempted to wrest a living from maize, sorghum, beef, or mutton—goods that eventually found their way to distant urban markets.

But those who acquired the ground under the guise of patriotism had withdrawn too rapidly. Once the giant De Beers Company managed to monopolize the diamond holdings of Kimberley, independent diggers started working their way up the course of the Vaal in pursuit of the alluvial diamonds to be found on the surrounding flood plain. From about 1910 to 1925, Bloemhof and the surrounding alluvial fields enjoyed an unprecedented if unevenly spread boom. Not only did the discovery of small, high-quality gemstones help revitalize parts of the local economy but the influx of diggers and their families created—for the first time—a sizable, albeit highly volatile, market for agricultural products within the triangle itself. It was only after the focus of the diggers' attention had shifted unequivocally to Lichtenburg in 1927 that the South-Western Transvaal slumped back into its more accustomed supine economic posture. When the tide of the Great Depression receded from the countryside in the mid-1930s, the region was left with scores of under-capitalized landlords, hundreds of *bywoners*, and several thousand "poor whites."

Badly scarred by this boom-and-slump economy and therefore deeply suspicious of all traders, grain merchants, diamond buyers, smart city lawyers, property speculators, and land companies, the triangle spawned three generations of militant republicans, religious fundamentalists, and radical populists. The region that saw the birth of the short-lived Boer Republics of Stellaland and Goshen in the nineteenth century dutifully provided its quota of die-hards (*bittereinders*) for the South African War of 1899–1902, thrust to the fore the quasi-millenarian "Siener" (Visionary) van Rensburg in the Rebellion of 1914, and produced the brand of tough-minded Afrikaner nationalists who, even before the political triumph of the mid-1940s, managed to enforce a short-lived economic boycott of Indian traders within the triangle in 1947.¹⁴

The newly formed South African state was deeply troubled by the existence of so large a reservoir of rural resentment and tried to siphon off militancy by providing the region with a distinctive political and economic system. In 1908, Wolmaransstad became the first district in the Transvaal to benefit from the presence of an agricultural cooperative when General J. C. Smuts's Het Volk government passed legislation to facilitate the establishment of the South-Western Transvaal Agricultural Co-operative or, as it was better known in Afrikaans, the SWTL. In a region with more than its fair share of badly under-capitalized Afrikaner farmers, the SWTL sought to pass on the economies of scale to the small farmer by introducing bulk purchasing schemes and by attempting to introduce a modicum of stability to wildly fluctuating grain prices.¹⁵ The state also released substantial tracts of crown

¹⁴ See, for example, D. J. Millar, "To save the 'Volk'! The 1947 Consumer Boycott of Indian Retail Traders in the Transvaal," unpublished B.A. (Honors) seminar paper, 1988, Department of Geography, University of the Witwatersrand.

¹⁵ See, for example, Suid-Westlike Transvaale Landboukoöperasie Beperk, *'n Halfeeu van Koöperasie, 1909–1959* (Leeuwardingstad, 1959).

lands to farmers within the triangle at reasonable prices after each of the two world wars, while the Land Bank made capital available to would-be agriculturalists at prices below those ruling on the financial markets elsewhere. Governments between the wars, especially those of General J. B. M. Hertzog, continued to make sure that the farmers of the region enjoyed access to the state at the highest possible levels. For a decade and half—from 1924 to 1939—General J. C. G. Kemp, one of the leading figures in the 1914 Rebellion and later the member of Parliament for Wolmaransstad, enjoyed a virtual monopoly of the portfolios of Agriculture and Lands in the government of the day.¹⁶

But, long before the fiery General Kemp's day—indeed, not long after the initial diamond discoveries had been made in Griqualand West nearly sixty years earlier—the South-Western Transvaal had also succeeded in attracting its first wave of black immigrants. Composed for the most part of unskilled wage laborers, woodcutters, transport riders, and crop-raising farmers, significant numbers of these newcomers were drawn into the triangle and the surrounding districts from Basutoland, the northern Cape Colony, the eastern Free State, and parts of the northwestern Transkei. Others came from even farther north and east. By the turn of the century, the area between the Vaal and the Harts was already noted for the wide variety of languages spoken by its African inhabitants, including Afrikaans, SeSotho, Shangaan, SeTswana, SiXhosa, and Zulu. The situation thus contrasted sharply with areas farther to the north and west, where SeTswana held sway.¹⁷ The hundreds of black families who established themselves in the triangle during this early period included several outstandingly successful grain and livestock farmers. Their ranks were further augmented when, in 1913, the infamous Natives' Land Act undermined sharecropping as a legal institution in the Orange Free State and pushed a second wave of African farmers across the Vaal and into the South-Western Transvaal. During the more "successful" decades after World War I, such as the 1920s and the 1940s, such names as the Marumos, Maines, Masihus, Seiphethlos, Tabus, and Tjalempes assumed almost legendary proportions for farmers of all classes and colors in the South-Western Transvaal.¹⁸

There were good reasons for this fame. Besides being talented crop-raising and stock-raising farmers, members of these large sharecropping households often possessed a little formal education and a formidable array of supplementary skills. In addition to his skill with the plow and the ox, for example, Kas Maine—the Transvaal-born son of a "first-wave" MoSotho immigrant—was also a qualified traditional herbalist and a more-than-competent blacksmith, carpenter, cobbler, saddle-maker, stonemason, tailor, thatcher, and transport rider. Most of these families had come to the Transvaal with a battery of farming equipment such as harrows, planters, plows, and wagons, as well as the necessary accouterments. But, above all else, it was these entrepreneurs' draft animal power and considerable livestock resources that placed them among the leading sharecropping families in an area dominated by sharecroppers. When Maine was pushed out of the triangle

¹⁶ On J. C. G. Kemp, see P. C. Du Plessis, ed., *Standard Encyclopedia of Southern Africa* (Elsies, 1972), 6: 324–25; or L. E. Neame, *Some South African Politicians* (Cape Town, 1929), 64–65.

¹⁷ See G. Relly, "Social and Economic Change among the Tswana in the Western Transvaal, 1900–1930" (M.A. dissertation, School of Oriental and African Studies, University of London, 1978), 5.

¹⁸ The lives of these sharecroppers, which have been systematically recorded since 1979, are preserved in the M. M. Molepo Oral History Collection, African Studies Institute, University of the Witwatersrand, Johannesburg. The evidence used in this essay is drawn almost exclusively from this source unless explicitly stated to the contrary. Out of comparative interest, see also T. J. Keegan, *Facing the Storm: Portraits of Black Lives in Rural South Africa* (Cape Town, 1988).

by the economic triumph of the tractor and the political muscle of Nationalist farmers in 1949, he had just completed a season in which his share of the harvest amounted to over one thousand bags of sorghum, and the family drove before it two spans of oxen, eight horses, fourteen donkeys, over forty head of cattle, and one hundred-fifty sheep.¹⁹ Willem Mosamo Masihuh, a "second wave" MoSotho immigrant extruded from the Orange Free State by the provisions of the Natives' Land Act, taught all six of his daughters to ride so they could help round up a flock of sheep that, even in the 1930s, numbered well over a thousand.²⁰ Nini Tjalemphe, another "second wave" immigrant, who hailed from the Herschel district of the Cape Colony, found his way into the triangle after a farming odyssey that had taken his family through Basutoland, the Orange Free State, and the Eastern Transvaal. By the early 1920s, this highly respected elder in the African Methodist Episcopal church had sent a daughter back to the Transkei for schooling and was the proud owner of scores of donkeys, horses, goats, and sheep, as well as more than two hundred head of cattle.²¹ The holdings of Mdebeniso Tabu, like Tjalemphe a "second wave" Xhosa-speaker who had entered the South-Western Transvaal from the Orange Free State, were even more impressive. In 1925, a herd of over four hundred cattle on Pienaarsfontein farm helped him earn sixteen hundred bags of sorghum from J. J. Meyer, the so-called Kaffir Corn King of the triangle.²²

Virtually the only thing that black men of this economic stature could not aspire to own within the confines of the rural universe was the land itself. They knew it, and it hurt. Kas Maine once warned the very same Meyer: "Baas [Boss] Koos, one day God will allow us to purchase land just like you, and I will hire you, and over-work you just as you are doing to me."²³ But, until that time, the position remained as the laconic Maine sketched it: "The seed was mine, the ploughs were mine, the oxen were mine. All was mine, only the land was his."²⁴ It was ownership of property that gave the white landlords of the district the edge over these black farmers who, in most respects, outfarmed them. And it was the need to gain access to land that drove black Africans into the sharecropping agreements that were at the foundation of the triangle's economy between the two world wars.²⁵

But, if this arrangement between badly under-capitalized white landlords and marginally better-off black farmers met the needs of the vast majority of the South-Western Transvaal's inhabitants for the better part of half a century, it also managed to alienate at least one class of rural producer.²⁶ Aspirant capitalist

¹⁹ M. M. Molepo Collection, tapes no. 130 and 131, interview with Kas Maine conducted by M. T. Nkadameng at Ledig, February 12, 1980.

²⁰ M. M. Molepo Collection, tape no. 606, interview with A. T. Seiphethlho conducted by E. Kgomo at Klipkuil, Makwassie District, December 8, 1987.

²¹ M. M. Molepo Collection, tapes no. 204, 205, 231, 479, 508, and 521, interviews with Kas Maine conducted by M. T. Nkadameng at Ledig, between July 2, 1980 and June 14, 1985. Also, interview with R. S. Meyer conducted by C. van Onselen at Devon, Eastern Transvaal, March 15, 1984.

²² M. M. Molepo Collection, tape no. 547, interview with T. Tabu conducted by M. T. Nkadameng at Lethapong, Taung, April 29, 1986; and tape no. 509, interview with Kas Maine conducted by M. T. Nkadameng at Ledig, February 16, 1985. See also interview with J. J. Meyer conducted by C. van Onselen at London, Schweizer-Reneke, June 11, 1984.

²³ M. M. Molepo Collection, tape no. 477, interview with Kas Maine conducted by M. T. Nkadameng at Ledig, June 26, 1984.

²⁴ For this and an early—and therefore incomplete—history of Kas Maine, see M. T. Nkadameng and G. Relly, "Kas Maine: The Story of a Black South African Agriculturalist," in B. Bozzoli, ed., *Town and Countryside in the Transvaal* (Johannesburg, 1983), 89–107.

²⁵ See H. Bradford, "The Industrial and Commercial Workers' Union of Africa in the South African Countryside, 1924–1930," (Ph.D. dissertation, University of the Witwatersrand, 1985), 240.

²⁶ The broad rise and decline of sharecropping on the highveld is documented in Keegan, *Rural Transformation in Industrialising South Africa*, 51–130.

farmers in search of cheap black wage labor disliked sharecropping agreements. As a self-styled "Progressive Farmer" from the Makwassie district pleading for farm reform shortly before the Natives' Land Act warned, "as long as landowners (they cannot be called farmers) are content to enter into partnerships with Kaffirs on the half-share system, the genuine farmer will never be able to get Kaffirs to work for wages." The author of this essay was concerned not only with the economic problems to which the dominant sharecropping system gave rise but also with the way this system informed the emerging social order. Thus, in the course of his venture into journalism, "Progressive Farmer" also linked two otherwise discrete issues by suggesting that, "so long as these landowners are willing and do not consider it 'infradig' to enter into partnership with niggers any amount of legislation will not stop the practice any more than it will stop white men from living with Kaffir women on the farms, as is so often the case. At the same time it would be a great step in the right direction if these people could be advertised in some way and exposed to public ridicule, and in some cases obloquy."²⁷ At the time of his writing, there were already several children growing up in the triangle who were the products of exactly such cross-color liaisons and at least some of them—including Ishmael Moeng and Mmerekhi Molohlanyi—went on to become prominent "black" or "colored" sharecroppers in their own right.

From "Progressive Farmer's" protest, it would appear that all was not well with the rural social order on the eve of the Natives' Land Act. Nor was this simply the old and familiar problem of the victorious colonizer exploiting his position to gain sexual access to the women of the vanquished. Had the author chosen to examine social relations between white and black *men* in the triangle, he would have found another, equally distressing cause for concern. The South-Western Transvaal's agricultural economy apparently facilitated a measure of cultural osmosis between landlords, sharecroppers, and *bywoners*, which, in turn, advanced notions of social equality in the countryside. It is to this evidence that we now turn our attention.

WRESTING A LIVING FROM THE LAND IN THE TRIANGLE has always been a hard and hazardous business. Because of the historical shortage of resident cheap black labor in the South-Western Transvaal, many white landlords had no choice but to do part of the more physically demanding work on the farm, such as plowing, planting, and harvesting. Hours of arduous labor spent together in the isolation of the fields tended to draw white landlord and black sharecropper closer together—albeit briefly. Kas Maine recalled how his older brother, Mphaka, while working with the Elliots in the early 1920s, had been soundly berated, after a hard morning's labor in the fields, for waiting for the arrival of the landlord before beginning a meal. "You say that you were waiting for the boss," Elliot admonished him, "but there is no boss here. We are all workers here and the meals brought here are for everybody to enjoy."²⁸

Likewise, the Seiphethlo family's high regard for Gert van Lill, of Witpan farm, was partly predicated on the same set of considerations: the white farmer's ability to join the sharecroppers for a hard day in the fields and then sit back and relax over a meal. As Andries Seiphethlo later recalled, "When we had finished ploughing that was

²⁷ "Farming Reform," *The Record of Klerksdorp and the Western Transvaal*, November 15, 1912, State Library, Pretoria.

²⁸ M. M. Molepo Collection, tape no. 381, interview with Kas Maine conducted by M. T. Nkadameng at Ledig, March 10, 1983.

it. We would then harrow the fields. After that we would take the hoes to cultivate and remove weeds right through the day. Once we had finished that we would congregate at somebody's home. Van Lill was so much like a black man. We would then leave for a friend's homestead at about dusk. From there you could go to van Lill's house, make yourself some food, eat and that would be it for the day."²⁹ The focus on physical exertion and the subservience of black and white alike before the demands of nature at the high tides of the agricultural year could bring a seasonal blur to the social distinctions that otherwise separated landlord from sharecropper.

The landlords, and occasionally even their wives, added their labor to the fields when the crops had to be picked. In 1928, on the Zorgvliet farm, a union-generated shortage of labor drove landlord Koos Reyneke and his bride of a only few weeks to join the Maine family in the fields and help pick maize until their fingers bled.³⁰ Across the district, on Witpan farm, Cornelius Du Toit's efforts in the fields during the early 1930s left a lasting impression on members of the Marumo family: "You would find Du Toit, taller than the BaTswana, busy harvesting. There he was, white skin and all, with a bag in his hands and his hands covered in blood from the way that the skin had been pierced by maize stalks. We would comment in amazement at this man who was so determined to get involved in harvesting although, strictly speaking, it was unnecessary for him to do so."³¹

Yet, for all the shared physical activity, by the end of the agricultural cycle, the vast majority of black sharecroppers had invariably undertaken far more manual labor than most of their white landlords ever dreamed of doing. Perhaps it was this fundamental inequity and the basic injustice of the system as a whole that informed African behavior when it came to threshing the crop. J. P. Pienaar, who in 1930 became the first man in the triangle to acquire a steam-driven Ruston and Hornsby thresher, clearly remembered the tension between the partners that he encountered at the five threshing points he had established in the Bloemhof district. Years later, his son Wikus also remembered how the sharecroppers "would play the gentleman" at such moments, refuse to assist the seasonal harvesting teams that came to the South-Western Transvaal from the reserve at Taungs, "arm themselves with notebooks," and check incessantly on both thresher and landlord to ensure they were not being cheated of their share of the harvest. Any mutual respect between landlord and sharecropper did not preclude the need for vigilance when the crop was divided.³²

But, if the better-off landowners and accumulating sharecroppers tended to move apart at this crucial juncture, the *bywoners*—by virtue of the work they performed and the underprivileged social and economic position they occupied in the rural order—were locked into a more permanent structural proximity to most of the blacks on the farms.³³ Andries Seiphetlho, for example, likened the position of Gert van Lill during the late 1920s to that of an ordinary African labor

²⁹ M. M. Molepo Collection, tape no. 607, interview with A. T. Seiphetlho conducted by E. Kgomo at Klipkuil, Makwassie District, December 8, 1987.

³⁰ Interview with J. C. Reyneke conducted by C. van Onselen at Olifantshoek, May 21, 1986; see Bradford, "Industrial and Commercial Workers' Union of Africa."

³¹ M. M. Molepo Collection, tape no. 609, interview conducted with M. Marumo by E. Kgomo at Lebaleng, Makwassie District, March 22, 1988.

³² Interview with L. T. Pienaar conducted by C. van Onselen at Schoonsig farm in the Bloemhof district, June 11, 1984.

³³ M. M. Molepo Collection, tape no. 607, interview with A. T. Seiphetlho conducted by E. Kgomo at Klipkuil, Makwassie District, December 8, 1987. On *bywoners* or white labor tenants, see, for example, H. Giliomee, "The Beginnings of Afrikaner Nationalism, 1870–1915," *South African Historical Journal*, 19 (1987): 119, 123, 131.

tenant—one rung down from that occupied by the sharecroppers. “He shepherded sheep, he herded cattle. He looked after all the animals and everything on the farm. He was fed just like you [a landlord] would feed a black person whom you owned.” “He lived,” Seiphethlo concluded, “just like a black man.”³⁴ Even lower down the ladder were the “poor whites” (*amaqaga*)—men like Gawie Vorster, a resident of Oersonskraal in the 1920s. He not only lived like most other blacks in the triangle but habitually addressed those men with whom he was on more intimate terms as “my brother” (*ngwaneso*).³⁵

Lest it be thought that such people were exceptional and their circumstances the result of the Great Depression, we should look at the experiences of the Maine family. As early as 1898, when the family was resident in the Schweizer-Reneke district, they encountered the widow Swanepoel and her youngsters, who lived in the black settlement (*stadt*) on Holpan farm. Not surprisingly, the three Swanepoel sons grew up speaking fluent SeTswana. Kas Maine’s close friendship with Hendrik Swanepoel when they were adults culminated in a most successful transport-riding venture.³⁶ Twenty years later, during World War II, Maine developed another association with a *bywoner* named Cornelius Fick, Hendrik Goosen’s foreman (*kneg*) on Klippan farm in the Bloemhof district. “He could speak SeSotho just like me,” Maine recalled, and, on at least one occasion, sharecropper and *bywoner* sided together in a dispute against the landowner, when Goosen apparently attempted to defraud Fick of a sheep that was owed to him. Moreover, when the *bywoner*’s son was arrested by the police in Johannesburg, it was to the black sharecropper rather than the white landlord that Fick turned for the necessary financial assistance to enable him to arrange an appropriate defense.³⁷ As late as the mid-1950s and in the twilight of his career as a sharecropper, one of Maine’s last farming ventures was with a “poor white,” J. J. N. Smit, nicknamed Dinta—“The Louse.” Even after the accession of the Nationalist government to power in 1948, poverty and the demands of agricultural production could, on occasion, still draw together those whom the law increasingly wished to force apart.³⁸ That many such cross-color friendships were cemented by similar class interests was not lost on the black sharecroppers. “Rich whites tried to pull you down,” Maine once observed, whereas “when they were poor we got on well.” Asked whether there were any whites whom he would venture to call “close friends,” the laconic sharecropper replied, “Man, there were many, people like Swanepoel and Reyneke. We got on well.”³⁹

Just as the need for a more broadly based physical effort at certain seasons by all able-bodied members on the farm could pull together under-capitalized white landowners and some of their black sharecroppers, so, too, could the celebration that marked the culmination of the agricultural year. On most farms in the South-Western Transvaal, a successful harvest was celebrated by a party attended

³⁴ M. M. Molepo Collection, tape no. 628, interview with A. T. Seiphethlo conducted by E. Kgomo at Klipkuil, Makwassie District, March 30, 1988.

³⁵ M. M. Molepo Collection, tape no. 628, interview with A. T. Seiphethlo conducted by E. Kgomo at Klipkuil, Makwassie District, March 30, 1988.

³⁶ M. M. Molepo Collection, tapes no. 511 and 519, interviews with Kas Maine conducted by M. T. Nkadimeng at Ledig, March 1, 1985, and May 11, 1985.

³⁷ M. M. Molepo Collection, tapes no. 491 and 512, interviews conducted with Kas Maine by M. T. Nkadimeng at Ledig, October 2, 1984 and February 2, 1985.

³⁸ M. M. Molepo Collection, tape no. 408, interview with Kas Maine conducted by M. T. Nkadimeng at Ledig, January 26, 1984.

³⁹ M. M. Molepo Collection, tape no. 511, interview with Kas Maine conducted by M. T. Nkadimeng at Ledig, March 1, 1985.

by all the men, women, and children resident on the property.⁴⁰ On these special occasions, when it was left to the white landlord to provide both the beast for slaughtering and the grain that the black sharecroppers used to produce a liberal supply of traditional beer, social relations were far more relaxed than at other times. High spirits, facilitated by the judicious intake of alcohol, especially among the males, allowed for more spontaneous interaction than usual and for the social order to be challenged and questioned through joking.

Yet, even at such festivities, structural constraints limited the extent to which the dominant order could be questioned. Mmakwetsi Marumo, for example, not only clearly recalled the dances and songs in which the immigrant BaSotho sharecroppers seemed to specialize but also that it was only the blacks who did the singing. The whites remained "seated in chairs because they did not sit in the Africans' places." Likewise, she suggested that the whites often limited their intake of alcohol and took home the greater part of their share of the beer because "they did not drink in front of the people."⁴¹ Despite these limitations, significant cross-cultural flows took place through the permeable membrane of ritual and seasonality. On the very occasion at which Mmakwetsi Marumo filed away these recollections, she also noted that everybody listened respectfully to the opening address in which the landlord paid tribute to God's generosity, as well as that it was traditional BaTswana beer (*BOJWALA*) that the whites took home to drink with obvious relish.

Celebrations on a large scale, such as the one sketched above, involved landlords and sharecroppers but also drew in labor tenants and wage laborers resident on the property. Perhaps it was precisely because of the scale of these gatherings and the cross-section of rural society they attracted that such celebrations often assumed an almost ritualistic aspect. Public meetings did not, however, exclude other less formal and more intimate exchanges from taking place in the shade of greater privacy. At Oersonskraal, where it was said of *bywoner* Gawie Vorster that "he behaved just like any of the local blacks"—"o itshwere SeSotho"—he met with the Seiphethos and other sharecroppers to drink *BOJWALA* throughout the year. Indeed, so frequently did this happen that some of the BaSotho immigrants felt that his behavior bordered on the improper and that it involved an element of social coercion (*kgang*). Others simply found him to be hospitable, while almost everybody was impressed by his fluency in SeSotho and a vocabulary that could amaze even native speakers. Many of the local Afrikaners found this degree of social integration distinctly threatening, however, and, according to Andries Seiphetho, they muttered among themselves that "Gawie Vorster was not a white man." He, in turn, had little respect for most whites in the district, whom he habitually described as "dirt" (*makgokgotsane*).⁴²

Kas Maine's experience of drinking with white men on the farms spanned more than two decades of a long life and took place in very different social settings. While at Kommissierust farm in the mid-1920s, Maine developed a particularly close relationship with his landlord Piet Reyneke's ebullient son, Willem. The older Reyneke, a respected church elder (*ouderling*) in one of the triangle's established

⁴⁰ M. M. Molepo Collection, tape no. 406, interview with Kas Maine conducted by M. T. Nkadimeng at Ledig, January 24, 1984.

⁴¹ M. M. Molepo Collection, tape no. 609, interview with M. Marumo conducted by E. Kgomo at Lebaleng, Makwassie District, March 22, 1988.

⁴² M. M. Molepo Collection, tape no. 628, interview with A. T. Seiphetho conducted by E. Kgomo at Klipkuil, Makwassie District, March 30, 1988.

churches, asked the sharecropper to escort his son and keep an eye on the younger man, as Willem set out by horse and cart to court the ladies of the Bloemhof district. On at least one occasion, however, the two friends simply used this time and space to visit the small hotel at London, where Willem purchased several bottles of brandy. The friends consumed some on the way home and then, suitably fortified, found their way back to the farm, where they hid the remaining spirits in a clump of eucalyptus trees. Unbeknownst to them, they had been observed by the elder Reyneke. Such was Maine's skill and standing as a sharecropper, however, that Piet Reyneke took no action other than to remove the offensive bottles discreetly at the first available opportunity.⁴³

But, if the younger Reyneke's behavior is to be written off as simply another example of the folly of youth in more tolerant times, across the way at Vaalrand, Piet Labuschagne—a mature and successful grain and wool farmer during the economically buoyant mid-1940s—had no such excuse. Labuschagne, like many other white farmers in the triangle, had a great liking for *BOJWALA* and delighted in taking a break from his work in the fields in order to share a drink with the hands. Nor did he hesitate to call at the Maine homestead for a drink whenever there was a chance that Leetoane Maine might have produced a freshly prepared supply of beer for her husband. On more than one occasion, Labuschagne arrived at the Maine home to find his partner and three or four other sharecroppers drinking while their youngsters were out in the fields at work. The amiable Piet would then laughingly rebuke the older men for their idleness, join the circle, and say, "Come on fellows, what's the problem? Pass me the calabash so that I can put it back."⁴⁴

Perhaps because he had enjoyed the hospitality of the Maine home so often, the landlord did not hesitate to reciprocate and entertain the sharecropper. One afternoon in mid-week toward the end of World War II, Labuschagne called Kas Maine out of the fields and invited him to drive with him to Bloemhof and search out a little amusement at an amateur boxing tournament. In the dusty little town, Labuschagne bought them some food and a bottle of brandy. The two friends drove to the edge of town, ate their meal in the privacy of the cab, and drank a good part of the spirits. Much cheered, they made their way back into the center of town where the son of a local railway worker was scheduled to box an outsider named van Rensburg, from across the river at Hoopstad. At the entrance to the tent, the two men from Vaalrand parted company, and, while Labuschagne made his way across the floor to link up with the white farmers at one end of the ring, Maine duly made his way to the other end, which was well patronized by sharecroppers and black farm workers. Eventually—and much to the delight of the home crowd—the local lad succeeded in knocking the visitor down. "He stood up, and the boy from Bloemhof threw another punch which again sent his opponent crashing to the floor and then everybody shouted: 'Hip, Hip, Hooray. All the way from Hoopstad to be defeated here!'" At the end of the evening's entertainment, the friends from Vaalrand reestablished contact, climbed into the truck, and drove back to the farm. Labuschagne was clearly most enthusiastic about the outing, Maine courteous and correct. Sometime thereafter, the landlord again came to the fields where the sharecropper was busy collecting maize and invited Maine to join

⁴³ M. M. Molepo Collection, tapes no. 381, 479, and 490, interviews with Kas Maine conducted by M. T. Nkadimeng at Ledig, March 10, 1983, July 10, 1984, and October 1, 1984.

⁴⁴ M. M. Molepo Collection, tapes no. 231 and 381, interviews with Kas Maine conducted by M. T. Nkadimeng at Ledig, July 27, 1980, and March 10, 1983.

him on yet another expedition into town. "Hey, let's go," Labuschagne shouted from the truck. As far as Maine was concerned, however, this insistent invitation jeopardized their joint enterprise. As the hard-working sharecropper recalled it many years later, "I refused to go and told him that I could not stomach that shit—sitting in a tent all day watching others do their work while we left our own unattended."⁴⁵

But few sharecroppers in the triangle were as single-minded and dedicated to farming as Maine. Most had fond memories of the many hours of leisure spent on the farms and elsewhere. While most of this recreation centered on beer-drinking or dancing to traditional BaSotho or BaTswana music, there were also times when the proceedings were clearly influenced by Afrikaner and other, even more distant, cultural practices. Andries Seiphetlho, for example, clearly remembered how he first met and courted his wife, Miriam Doek, at the all-night dances held on Koos Hoffman's section of Heuningkrans farm in the period 1922–1924. On most weekends, people from miles around would gather at the home of the old Koranna Flip Jonker to drink, sing, and dance to the accompaniment of the autoharp, banjo, and concertina, which the older Jonker children played with considerable distinction. Some of their tunes were SeTswana ditties like "Serantl-hatlha," which celebrated the achievements of a homemade guitar. But most popular at the time were three exotic dances far from the mainstream of any "black" culture—a Boer "waltz" called "Tannie en die Roos" (Auntie and the Rose), the mazurka, and a lively hop in which the men and women lined up to perform the "Cobben."⁴⁶

In an area of marginal rainfall, while the inhabitants of the triangle clearly enjoyed their fair share of recreation, the vagaries of nature always left them with sufficient time to contemplate more serious matters. And, if there was one set of ideas that connected the vast majority of these rugged and sometimes extremely competitive individualists, it was their adherence to Christianity in an area where Islam and Judaism were associated with merchants and traders—positions of low esteem in the eyes of resentful white farmers.

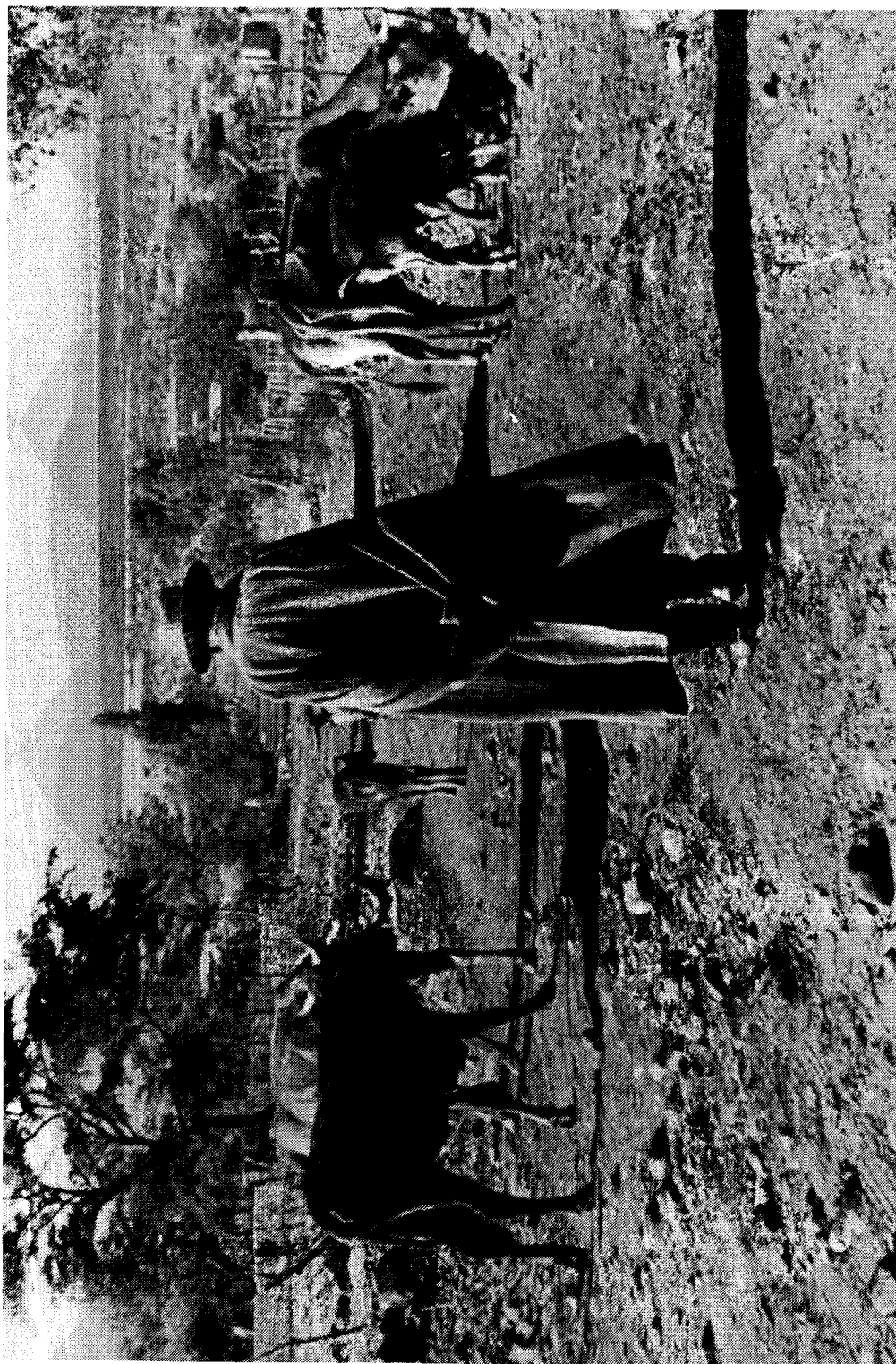
CHRISTIANITY, IN VARIOUS GUISES, appeared early on in the South-Western Transvaal.⁴⁷ Indeed, it was one of the first areas in the southern African interior where Africans were systematically exposed to the activities of the missionaries—a field of endeavor in which the Methodists and Lutherans were particularly prominent. On the farms, however, the loyalty of Afrikaner landlords lay with one of two "traditional" churches: the theologically ultra-conservative Nederduits Gereformeerde Kerk (NGK) or the conservative Hervormde Kerk.⁴⁸ While some of

⁴⁵ M. M. Molepo Collection, tape no. 407, interview with Kas Maine conducted by M. T. Nkadameng at Ledig, January 26, 1984.

⁴⁶ "Field Report" by E. Kgomo on an interview conducted with A. T. Seiphetlho conducted by E. Kgomo at Klipkuil, Makwassie District, March 27, 1988; see also M. M. Molepo Collection, tape no. 631, interview with A. T. Seiphetlho conducted by E. Kgomo at Klipkuil, Makwassie District, May 29, 1988. Some of the background to the use of these musical instruments—although not in a BaTswana setting—is to be found in J. Clegg, "The Music of Zulu Immigrant Workers in Johannesburg: A Focus on Concertina and Guitar," *The International Library of African Music*, Grahamstown, Papers presented to the First Symposium on Ethnomusicology, 1980.

⁴⁷ A brief outline is to be found in B. A. Pauw, *Religion in a Tswana Chieftdom* (London, 1960), 1–11.

⁴⁸ See, for example, T. Dunbar Moodie, *The Rise of Afrikanerdom: Power, Apartheid and the Afrikaner Civil Religion* (London, 1975), 60–61, on the different roles of the Dutch Reformed churches.



KAS MAINE, age ninety, in the small rural town of Ledig. Photographed in 1983 by David Goldblatt.

the black farm workers and labor tenants followed their landlords into these institutions, others found a home in one of the many other churches or occasional African Zionist sects to be found in the triangle. Higher up on the socioeconomic ladder, some of the sharecroppers found a foothold in the Lutheran church, but, above all others, the African Methodist Episcopal church, with its black American connections, won the allegiance of the most successful sharecroppers in the years between the two world wars.

Within the triangle, as elsewhere in the country, organized religion found that it could somehow accommodate God, the social order, and racial segregation in a set of structures that apparently did violence to neither logic nor theology. The only real exception was to be found across the way at Oersonskraal, where the iconoclastic Gawie Vorster was said to be an elder in a Pentecostal church that held racially integrated services. This practice, the sharecroppers were quick to recall, earned him the strong disapproval of not only Reverend Thompson of the Lutheran church at Wolmaransstad but also of most of his Afrikaans-speaking neighbors.⁴⁹

For all the dark mutterings about the social subversion that was bound to follow from any officially sanctioned form of racial equality in church or state, Afrikaner behavior—in the privacy of the home and the isolation of the farm—was often far more flexible. At Hartsfontein farm, W. A. Nieman, a tough, irascible founding member of the local branch of the National party, an *ouderling* in the NGK, and a man who would never deign to offer “Ou Vel” (Old Hide) Gabbe the Jewish trader (*smous*) a bed for the night, found nothing incongruous about inviting the farm hands into his dining room for the nightly prayer meeting (*huisgodsdiens*).⁵⁰ Nor was it simply a question of some gatherings always being held in the landlord’s idiom or in the innermost sanctuary of his domain. At Kommissierust, during the drought-stricken 1920s, Piet Reyneke on more than one occasion attended an open-air “Day of Prayer” presided over by the Reverend Makhwanazi, who happened to have a following among the sharecroppers and labor tenants on the property. At this and other services conducted in two or more African languages, the landlord often called on Kas Maine’s mother, Motheba, a stalwart in the women’s guild of the AMEC, to lead the gathering in a prayer. It was invariably rendered in Afrikaans at least as good as Reyneke’s fluent SeTswana. The landlord, in turn, put his command of SeTswana at the disposal of SeSotho-speakers who had trouble in following the vernacular.

Likewise, it was not unusual for sharecroppers, labor tenants, or wage laborers to be invited to funerals or marriages conducted on the farm. But, given the form and public nature of these rituals, embedded in the culture of the politically dominant group, such occasions were governed by a strict code of racial etiquette. Blacks attending the gatherings were expected to make their presence as unobtrusive as possible.⁵¹ Still, it is reasonable to assume that it was precisely at such events that blacks found themselves exposed to some of the changing dimensions of Afrikaner culture. If it suited them, some elements were later incorporated into their own practices, such as “Tannie en die Roos.”

Although the major cultural exchanges within the triangle took place along the

⁴⁹ M. M. Molepo Collection, tape no. 628, interview conducted with A. T. Seiphetlho by E. Kgomo at Klipkuil, Makwassie District, March 30, 1988.

⁵⁰ Interview with R. J. Nieman conducted by C. van Onselen at Brits, June 12, 1985.

⁵¹ M. M. Molepo Collection, tape no. 381, interview with Kas Maine conducted by M. T. Nkadameng at Ledig, March 10, 1983.

political fault lines of colonial power—that is, from white landlords to black tenants—some of the traffic moved in the opposite direction. As a result of the prevailing poverty, relative isolation, low levels of education, and the class proximity of the poorer landowners and *bywoners* to sharecroppers and labor tenants, some of the whites in the South-Western Transvaal were strongly influenced by elements of African culture. The Afrikaner churches were aware of this process of cultural osmosis and, in response, as one astute observer has noted, “took a strong stand against magic and folk medicine. Between 1906 and 1911 there seems to have been an epidemic of such things in many parts of South Africa. Reports of people being censured for ‘magic,’ ‘witchcraft’ or consulting ‘sympathetic doctors’ came in from a number of congregations and the issue was discussed at various Provincial Meetings of the Church. So great was the problem that the Synod decided to produce a brochure condemning magical practices and giving Biblical grounds for their avoidance.”⁵² There is little to suggest a dramatic falling off in such practices after the issuing of a pamphlet that—in the triangle at least—many whites would have had great difficulty reading. Novelist Bosman constantly returned to the theme of “witchdoctors” and their supposed powers in his Marico fiction. When C. Louis Leipoldt published his experiences in *Bushveld Doctor* (1937), he felt it prudent to devote a section of the work to a discussion of bush magic. When Isadore Frack set down his reflections on being a South African doctor, some seven years later, he too devoted a chapter to “fooling the farmer.”⁵³

Landowners and *bywoners* in the South-Western Transvaal were no more immune to this “cultural contagion” than their *bushveld* cousins who happened to live a little farther north. Throughout the 1920s, Kas Maine, in his role as a prominent local herbalist, routinely protected his fields from the onslaught of redbilled quelea and other grain-eating birds by spreading a magic potion around the perimeter of his fields at the start of each new season. His success was evident not only to his landlord but to several other white farmers in the district, who called on him in private to perform the same ritual in their fields. This he did for the standard fee of five bags of grain discreetly handed over at the end of the season. Forty years later—in the mid-1960s—his nephew, Hoai Maine, who had made something of a specialty of this practice, was still being called upon by prominent white farmers in the Bloemhof and Schweizer-Reneke districts to perform the ritual.⁵⁴ While some landowners were interested in traditional magic that could be linked to agricultural production in an area where nature was notoriously capricious, *bywoners*, “poor whites,” and ordinary blacks on the farms were eager for the herbalist to restore their health. Maine used a potent combination of Christian symbolism and traditional herbs to cure his friend and business partner Hendrik Swanepoel’s chronic backache.⁵⁵ Piet van Schalkwyk was possibly more typical of the white patients whom Kas Maine treated during the 1920s. A “poor white” drawn from the sprawling Bloemhof diamond diggings, van Schalkwyk, or, to give

⁵² I. Hexham, “Modernity or Reaction in South Africa: The Case of Afrikaner Religion,” paper presented to the Consultation on Modernity and Religion, University of British Columbia, December 15–18, 1981, 25.

⁵³ H. C. Bosman, *Jurie Steyn’s Post Office* (Pretoria, 1971); C. Louis Leipoldt, *Bushveld Doctor* (London, 1937), 202–23; and I. Frack, *A South African Doctor Looks Backwards—and Forward* (Johannesburg, 1943), 17–136.

⁵⁴ M. M. Molepo Collection, tapes no. 325 and 486, interviews with Kas Maine conducted by M. M. Molepo, M. T. Nkadameng, and E. Msimango at Ledig, May 13, 1982, and August 29, 1984. See also N. Mokgatle, *The Autobiography of an Unknown South African* (London, 1971), 32–33.

⁵⁵ M. M. Molepo Collection, tape no. 381, interview with Kas Maine conducted by M. T. Nkadameng at Ledig, March 3, 1983.

him his SeTswana name, "RaMotokwane" (the Father of Dagga, or Marijuana), had contracted what he thought to be an embarrassing, sexually transmitted disease. He was reluctant to be treated by the local general practitioner, who might feel disposed to have a word with the minister in such a small town. Maine, however, arrived at a different diagnosis and, by administering the correct herbal remedy, succeeded in draining off the fluid that had collected in the man's scrotum.⁵⁶

The majority of sharecroppers and black farm laborers who consulted Kas Maine during the 1920s and 1930s, such as his white patients and the occasional Asian and "colored" storekeeper, came in search of the traditional herbs on which the herbalist's (*ngaka*) reputation was based. But, even on the supposedly firmer ground of traditional Sotho or Tswana custom, there were often subtle cultural interchanges taking place. Maine, like most of his professional colleagues, was not beyond prescribing cheap, efficacious patent medicines to his black patients should the occasion or the ailment demand it. Besides the usual range of laxatives and "Dutch remedies" such as "Staaldruppels," his collection of patent medicines at the time included other popular items such as "Blue Butter," "Buchu Rub," "Dr. Kiesow's Essence of Life," "Evan's Throat Pastilles," and "Zam Buk Ointment." In mysticism, health, and healing—as in so many other areas of life in the triangle—it was often an untidy and ragged line that separated black cultural practices from white.

IN MOST OF SOUTHERN AFRICA, including the South-Western Transvaal, a complex, unwritten code of racial etiquette governed the daily patterns of social interaction between the politically dominant whites and the large mass of oppressed blacks. And, since one of the most important implicit functions of this code was both to mirror and reproduce the colonial power structure in interpersonal relations, much of the onus for acquiring the appropriate racial etiquette was on blacks.⁵⁷ Kas Maine, who undoubtedly mastered the code in his fifty years on the land, felt that, if a black man were to survive in the triangle, he had to be like "a chameleon among the Boers."⁵⁸ In his experience, success was predicated on an ability to read the subtle ways in which space, age, gender, language, dress, gesture, and tone—among dozens of other variables—could affect interpersonal relationships across the color bar. A man needed to understand that there was a world of difference in the way he transported a poor Afrikaner farmer's wife like Mrs. Willem Griesel to the local trading store by cart and the way he escorted the affluent, English-speaking Mrs. A. V. Lindbergh by coach on her shopping expeditions to "Paradise Brothers" in Wolmaransstad. A detailed exploration of that code in its entirety would make for a fascinating and lengthy study in itself. Here, I will focus very briefly on the extent to which racial etiquette in the triangle was predicated on considerations of class. What, in the broadest possible terms, characterized behavior between black and white males in an economically marginal area where

⁵⁶ M. M. Molepo Collection, tape no. 381, interview with Kas Maine conducted by M. T. Nkadameng at Ledig, March 3, 1983.

⁵⁷ The role of such codes in governing interracial behavior has been well documented in the American South. See, for example, B. W. Doyle, *The Etiquette of Race Relations in the South* (New York, 1971); or C. L. Flynn, Jr., *White and Black Labour; Caste and Class in Late Nineteenth Century Georgia* (Baton Rouge, La., 1983), 15.

⁵⁸ M. M. Molepo Collection, tape no. 381, interview with Kas Maine conducted by M. T. Nkadameng at Ledig, March 10, 1983.

under-capitalized landowners, sharecroppers, and *bywoners* were perhaps pushed into a greater social and economic proximity than elsewhere in the South African countryside?

Kas Maine was of the opinion that this difficult terrain was more easily negotiated where the scythe of poverty had cleared the way for humility and the prospect of genuine economic cooperation. Surveying the nature of his relationship with whites on the land between the two world wars, he observed, "Men like Swanepoel were not rich Boers, they were poor and they liked us. Those who were rich and of some social standing, like Meyer and Nieman, were bad." As a rule of thumb, it was those with least capital "who liked and cared for blacks because they benefited from their association with them. They knew that we would pick them up and make them rich."⁵⁹ It was largely because of this class affinity that Maine experienced little difficulty in getting along with Piet Reyneke as a landlord or Hendrik Swanepoel as a partner in their transport-riding venture. On the long grain hauls to the rail sidings located at the very margins of the triangle, he and Swanepoel worked together at every phase of the job, shared meals from the same *kaffirpot*, and slept beneath the wagon at night.⁶⁰ Yet, even with these men whom he counted among his closest friends, Maine was often aware that their relationship was testing the limits of the code and that it was therefore sometimes necessary to defuse any residual tension with some humor. When approaching Piet Reyneke with a request to release the harvesters from the fields, "I would say—'Hey, take that pipe out of your mouth.' [Laughter] Then he would say; 'What!' and I would say: 'Take that pipe out of your mouth, you are talking with a pipe in your mouth. Look here . . .'" However, he would not feel threatened and simply say: 'Lord!'"⁶¹

Other, more prosperous landowners, however, were distinctly uncomfortable when dealing with the sharecroppers. Take, for example, Maine's experience in dealing with J. J. Meyer, the upwardly mobile "Kaffir Corn King" of the triangle. "When I approached him I would say: 'Hello Baas Meyer.' He would then shake my hand very rapidly so that other Boers could not see and say: 'Yes, morning, you arsehole, so you are still alive!' And I would say: 'Yes Baas.'"⁶² Maine's relationship with Hendrik Goosen—a poorer and somewhat eccentric landowner with whom he worked in the early 1940s—was very different. If Goosen saw a black man approaching, he would readily extend his hand and say good morning; "he did not care whether other whites were watching him."⁶³

Because of the existence of this elaborate code of racial etiquette, black sharecroppers vividly recalled transgressions by white landowners. When Willem Griesel, Piet Reyneke's unashamedly racist brother-in-law, arrived at the family farm to bury his wife, the working people on the property were shocked to discover that black mourners were not to handle the coffin or to mix with the white guests on the short journey to the cemetery. But, here, the implacable Griesel had to give way before the people's expectation that they be allowed to place the customary

⁵⁹ M. M. Molepo Collection, tape no. 511, interview with Kas Maine conducted by M. T. Nkadimeng at Ledig, March 1, 1985.

⁶⁰ M. M. Molepo Collection, tape no. 381, interview with Kas Maine conducted by M. T. Nkadimeng at Ledig, March 10, 1983.

⁶¹ M. M. Molepo Collection, tape no. 511, interview with Kas Maine conducted by M. T. Nkadimeng at Ledig, March 1, 1985.

⁶² M. M. Molepo Collection, tape no. 381, interview with Kas Maine conducted by M. T. Nkadimeng at Ledig, March 10, 1983.

⁶³ M. M. Molepo Collection, tape no. 381, interview conducted with Kas Maine at Ledig, March 10, 1983.

spade of earth on the coffin of the deceased once it had been lowered into the grave.

Although technically outlawed by the provisions of the Natives' Land Act in 1913, interracial sharecropping was widely practiced in the South-Western Transvaal. From the turn of the century until the arrival of the tractor in significant numbers in the mid-1940s, black patriarchs commanded both the draft oxen for plowing and family labor for harvesting that somehow eluded chronically under-capitalized white landlords. In all his fifty years in the triangle, Kas Maine could recall only one instance of a white landlord being prosecuted for sharecropping—and that was hearsay. Local representatives of the South African state chose to ignore the widespread practice of sharecropping in a part of the country that, from well before the South African War and for at least six decades thereafter, provided a political home for radical white populists.⁶⁴ Yet, for all this flouting of the law by white fundamentalists, many if not most landowners lived in fear of prosecution. A few, like J. J. Meyer, took the precaution of issuing their partners false written contracts suggesting that the scores of blacks to be found on their properties were labor tenants rather than sharecroppers. Many more simply told their black partners to lie about their contractual arrangements if questioned by inquisitive outsiders. And, in the 1930s, a minority of whites took a step away from sharecropping by sharing labor-time rather than crops with their black tenants.

Whatever they did, the uncomfortable fact remained that the vast majority of the triangle's inhabitants was living an economic lie. Perhaps it was this situation at least as much as the traditional suspicion of rich, powerful city lawyers and courts that informed the rural search for natural justice. There were small court buildings at Bloemhof, Schweizer-Reneke, and Wolmaransstad, but the rich life of Kas Maine reveals a search for justice on land dominated by a sharecropping economy. While resident on Vaalrand farm in the early 1940s, Maine and the members of his family became close friends with one of the other sharecroppers on the property—a fellow MoSotho named Thloriso Kadi. When Thloriso's oldest son, Padimole, decided to get married, the Kadi family not only chose Maine to act as an intermediary with the bride's family but asked him to assist their relatives—the Marumos—with the catering arrangements for what was, in triangle farming circles, something of a society wedding.⁶⁵ The wedding, at which Padimole's young bride wore a white dress and a veil made by an Afrikaner dressmaker, was attended by representatives of several of the leading black sharecropping families as well as the landlord, Piet Labuschagne. The occasion was undoubtedly a great success, and, in the months thereafter, Kas Maine's friendship with Thloriso Kadi became even firmer. Padimole, however, apparently found his marriage to be less than satisfactory since, not long afterward, he developed a liaison with Maine's oldest daughter, Moroosi, who soon discovered that she was pregnant.

This unfortunate development angered Kas Maine, a traditionalist in such matters. Maine took his complaint and a demand for suitable compensation to his friend Thloriso who—after consulting Padimole—denied any Kadi responsibility

⁶⁴ See, for example, the career of that fiery Afrikaner patriot, Reverend E. J. van der Horst, who was elected to Parliament in 1915 to represent the Wolmaransstad district and who, in 1920, was forced to abandon the church only after a lengthy legal wrangle. See *Afrikaanse Kultuurvereniginge, Afrikaanse Kultuur Almanak* (Johannesburg, 1980), 73. Perhaps it is also significant that this was among the first rural constituencies in which the new Conservative party made political headway in the 1980s.

⁶⁵ These events are reconstructed from the M. M. Molepo Collection, tape no. 338, interview with Kas Maine conducted by M. T. Nkadameng at Ledig, August 29, 1982; and tape no. 395, interview with L. D. Masihu conducted by M. T. Nkadameng at Molelema, Taung, September 9, 1983.

for the daughter's condition. This denial produced great acrimony between the families and, some allege, culminated in an ugly confrontation and a stick fight between the two patriarchs. When this encounter proved inconclusive, Maine had the local scribe, Lerata Masihu, write Thloriso a letter summoning him to appear at a traditional court (*kgotla*), which, in the absence of any resident BaSotho chief, would be presided over by their landlord. This the Kadis readily agreed to. When Padimole again denied his role in the affair, Piet Labuschagne deferred judgment until such time as the child was born so that both families could inspect the infant and see if its physical features did not reveal the father. This decision eventually produced an amicable settlement between the two BaSotho families, who accepted the verdict of a white landlord who had been called upon to pass judgment in what was, to all intents and purposes, a "traditional family court."⁶⁶ The baby was named Johanna in honor of Labuschagne's wife.

The search for their own justice, the desire to avoid the established courts, and a respect for the standing of black litigants were not confined to cases between sharecroppers. Magistrates in the South-Western Transvaal were sometimes drawn beyond the confines of their formal office to intervene in cases between landlords and sharecroppers. The shrewdness, farming experience, and economic standing of sharecroppers in the triangle ensured that their pleas for justice were taken more seriously than those of other black labor tenants or mere wage laborers. For example, in the late 1930s, Kas Maine entered into a sharecropping agreement with an impoverished *bywoner*, Koos Klopper, who had hired a section of Klippan farm in the Bloemhof district. Unfortunately, the portion of land occupied by the partners was unfenced, and, for Maine, this led to serious problems with his closest neighbor, the dynamic but agrestic farmer Hendrik Goosen.

One morning in 1937, sixteen of the sharecropper's cattle strayed into his neighbor's maize fields. Goosen, justifiably angered by this destructive intrusion, instructed his laborers to round up the cattle and impound them in his *kraal*. He then promptly left for Bloemhof, where he had business to attend to. As soon as the Maine's oldest boy, Mmusetsi, discovered what had happened, he informed his father, who made his way across to the white farmer's land, only to discover that he was away in town. In the late afternoon, Maine returned to Goosen's, where the farmer informed him that he would release the cattle on the condition that the sharecropper paid him compensation of three shillings per beast. When Maine told him that he did not have the ready cash, Goosen said that he would instead accept three bags of sorghum, which the sharecropper duly handed over. Being aware of both Goosen's reputation for trickery and the power of the written word in a world where the law and literacy often went hand in hand, Maine also insisted on Goosen issuing him with a receipt proving that he had met his commitment. This the farmer did.

During October of the following year, however, the roles were reversed when sixteen donkeys and fourteen unattended oxen belonging to Goosen invaded the sharecropper's fields. Like the white farmer before him, Maine ordered Mmusetsi to round up the cattle and place them in his *kraal*, and he sent a message across to Goosen to inform him of what had transpired. Goosen sent back a letter, written in Afrikaans, which the timid Koos Klopper read to Maine: "Retain one ox and I will come across to assess the damage. Release the other animals so that they may be

⁶⁶ These events are reconstructed from the M. M. Molepo Collection, tape no. 381, interview with Kas Maine conducted by M. T. Nkadimeng at Ledig, March 3, 1983.

used for ploughing.” The sharecropper then set the animals free, holding back only a bluish hind-ox named Bloubank. But the canny Goosen realized that, if the ancient law of the Medes and the Persians was to prevail, he owed Maine the better part of £5 and therefore failed either to collect the ox or to inspect the damage that his animals had done to his neighbor’s fields. Maine kept the ox among his own animals for more than two months.

On Christmas Day 1938, Kas Maine had just slaughtered a sheep for a small family celebration when he was informed that fourteen of Goosen’s animals had again invaded his fields, where the new season’s crops were already standing knee-high. Once again, Mmusetsi was sent to round up the animals and impound them while Maine sent a message across to the farmer. But, this time, perhaps still smarting from the loss of “Bloubank,” Goosen sent back a note he thought would confound the sharecropper. Its message and sub-text could not have been simpler: “I am not interested in assessing the damage; do with the animals as you please.”

The following morning, Maine showed this abrasive note to an increasingly anxious Klopper, who, fearing Goosen’s eventual wrath, advised the sharecropper to take the animals to the nearest municipal pound. With the assistance of his friend Hendrik Lefifi and his son, Kas Maine set off in the direction of Sewefontein for the long journey into town. On the outskirts of Bloemhof, however, the three black men were overtaken by a party of whites—Hendrik Goosen, his father, and a younger brother—who asked the sharecropper where he thought he was taking the animals. When Maine told him they were on the way to the pound, Goosen asked him if he realized whose animals they were, to which the sharecropper responded that he had not the faintest idea to whom the beasts belonged. “Today you will find out,” an irate Goosen warned him; “By tonight you and the cattle alike will be in the custody of the police.”

The posse of whites then drove off in the direction of the Bloemhof police station, where Goosen allegedly told the officer on duty that it was Mmusetsi who had been responsible for driving the cattle into the sharecropper’s fields and that, because the older Maine had taken illegal possession of the stock, he should be charged accordingly. The police, it would appear, were less than convinced by this rather unlikely story. When Kas Maine and his entourage arrived at the Charge Office and asked for the farmer’s cattle to be impounded, they were told to wait at a nearby window where the magistrate would attend to them. When the magistrate eventually appeared, he asked the sharecropper to provide him with the background to the problem. Maine started by handing him the receipt for three bags of grain that Goosen had issued him on the occasion when his cattle had invaded the white farmer’s property during the summer of 1937. “When he had finished reading that one, I handed him the second one, the one that suggested that I keep back one beast until such time as he had time to come across and assess the damage.” When the magistrate still expressed some puzzlement, Maine handed him the third and decisive note in which Goosen suggested that he was unwilling to assess any further damage and that the sharecropper was free to do with the cattle as he pleased.

Once the magistrate had read this document, he ordered that Goosen’s cattle be impounded, and Kas and his helpers drove the animals toward the pens. At the pound gate, however, they were accosted by yet another policeman who demanded to know why it was that the white farmer’s cattle were being impounded. Maine, by then well aware of the growing strength of his position, again went through his well-rehearsed routine with the three notes. The police discussed the case among themselves and, after Maine had paid the standard fee of a shilling a beast,

reluctantly let the animals into the pound. Before Kas Maine could leave, however, the officer in charge asked the sharecropper to describe where exactly on the farm his fields were situated so that he could dispatch two men on motorcycles to inspect the damage done to the maize. "I directed them well, and they departed on two steambikes." At Klippan, the police interrogated one of Goosen's many black tenants, who confirmed the sharecropper's version of the story, and the police then returned to Bloemhof, where they told Maine to present himself again at the pound on the following morning.

"On the following day," Kas Maine later recalled, "I drove a cart into town and when we arrived they told me that they had seen the damage. They asked how many people had driven the cattle to the pound and I told them that there were two of us. They calculated the distance from Klippan to Bloemhof and paid us one pound five shillings each for bringing in the animals." The police then told him to allow two days to elapse and report back to the police station. What exactly happened in the interim will probably never be known, but, presumably, the police intervened to persuade Goosen where his interests lay. As Maine later told the story:

When we arrived there they told me that from the first two documents it was clear that he had impounded my cattle for straying onto his land and, although I did not own any land, I had impounded his cattle when they had invaded my plot. From the third document it was clear that I had again taken possession of his cattle. They then handed me ten pounds, saying that I should pay the person who had helped me drive the cattle to the pound. I took the money and paid Hendrik Lefifi a further two pounds. He added it to what I had given him on the first occasion and he ended up with three pounds and five shillings. I took what remained and put it in my pocket. I then asked them whether the matter would still be referred to the courts. They told me that the matter was settled.

In addition to paying compensation, however, the unhappy Goosen was also called upon to pay a substantial fee to have his cattle released from the pound.⁶⁷

Despite this dispute—and a still later round of legal conflict with Goosen revolving around the alleged theft of a sheep—Maine eventually joined forces with the landlord in a very successful sharecropping and stock-speculating venture in which both parties participated in some dubious ethical practices. The dynamics of their tortured relationship, even when refracted through the prevailing racist legal order, could hardly be characterized as a simple derivative of some semi-feudal order in which white master inevitably lorded it over black serf. In the South-Western Transvaal, as elsewhere, the scales of justice were suspended above the fulcrum of class. In the sharecropping economy of the triangle, there was no guarantee that white landowners would invariably triumph over black sharecroppers.

MY BROADER CONCLUSION—that everyday interactions between white farmers and black sharecroppers on the highveld in the arenas of production, recreation, religious life, health, and justice had undertones of social equality—does not square easily with some existing interpretations of South African rural history. Indeed, in the most influential article in the field, the author of "The Development of Capitalism in South African Agriculture" denies the very existence of crop-sharing arrangements after the passage of the Natives' Land Act of 1913. He suggests that there is probably little point in looking farther north or west for the practice after that date since, "in the Transvaal,

⁶⁷ M. M. Molepo Collection, tape no. 381, interview with Kas Maine, conducted by M. T. Nkadameng at Ledig, March 3, 1983.

labour tenancy was deeply rooted and the most prevalent form—indeed in the early 1930s—the *only* form of acquiring farm labour.”⁶⁸ Kas Maine, however, saw things very differently: “The only ones who did not plough on the halves were those who did not possess a span of oxen or donkeys. The Boers in the triangle wanted anyone who owned a span to plough on the halves.”⁶⁹

Nor were such arrangements promptly terminated when the Marketing Act of 1937 came into force to ensure white landlords a guaranteed minimum price for their maize. This new stability enabled the better-off among them to invest more readily in the gasoline-driven tractors that eventually freed them from their dependence on black sharecroppers’ oxen and labor. When World War II disrupted the supplies of imported agricultural machinery and equipment, it gave the practice of farming-on-the-halves a further—albeit short—lease on life. Tom McLetchie, a crusty, semi-literate former justice of the peace in the Schweizer-Reneke district, probably expressed the frustration of many of the smaller and less successful farmers in his letter of complaint to Colonel Deneys Reitz, minister of Native Affairs in the Smuts government, in 1940:

What about some native labour? There is plenty for all our requirements but it is badly distributed. Those who do little work and the very many who plow on shares and are practically on social equality with them in everything but name and shrieking “Segregation” all the time, have surplus labor galore therefore the natives flock to such farms where they work for only a few weeks in the year and hold illegal Beer Drinks almost constantly. Suggest that before the end of session, a law be forced thro both Houses, making it illegal under dire penalties, for any white landowner or occupier to have on his farm any native possessing a Plow and Draft animals. Then, there will be some hope for a White South Africa.⁷⁰

Given the persistence and pervasiveness of sharecropping as an economic institution in the South-Western Transvaal during the first half of the twentieth century, it becomes easier to understand why, in an area notable for the dominance of racist republican ideas and populist notions among its whites, a study of day-to-day behavior should yield evidence of a surprising degree of tolerance and flexibility in social relations. Contracts governing farming-on-the-halves not only gave African tenants a measure of protection against unbridled racist behavior by economically vulnerable Afrikaner landlords but also endowed blacks with a positive sense of dignity and value. “Boers who practised sharecropping did not pressurise or push sharecroppers around,” noted Maine. “They did not say; ‘Hurry, hurry.’ No! You worked independently, you were not dependent on them for your food. They merely owned the land, and their only concern was to share in the harvest.”⁷¹

Even though propinquity on the land helped to moderate behavior and draw landlord and tenant closer together, it also served as a painful reminder of how close, in social and economic terms, badly under-capitalized white farmers were to black sharecroppers in what was, and still remains, a colonial order. Indeed, were it not for their ownership of the land and their privileged political status, white

⁶⁸ These observations are drawn from Morris, “Development of Capitalism in South African Agriculture,” 294–95, my emphasis.

⁶⁹ M. M. Molepo Collection, tape no. 231, interview with Kas Maine conducted by M. T. Nkdimeng at Ledig, July 27, 1987.

⁷⁰ State Archives, Pretoria, NTS vol. 2207, file 354/280, T. McLetchie to Col. D. Reitz, May 5, 1940. I am indebted to Dr. H. Bradford, who drew this letter to my attention. For McLetchie’s experience in the triangle, which dated back to shortly after the South African War, see H. P. Marée, *Die Geskiedenis en Ontwikkeling van die Dorp en Distrik van Schweizer-Reneke, 1870–1952* (Schweizer-Reneke, 1952), 15.

⁷¹ M. M. Molepo Collection, tape no. 400, interview with Kas Maine conducted by M. T. Nkdimeng at Ledig, November 29, 1983.

landlords and black tenants would have been hard to distinguish. It was precisely because they were "practically on terms of social equality" with their sharecroppers that white farmers in the triangle were "shreiking for 'Seggregation.'"

In attempting to come to grips with the complex realities that govern the process of accommodation in this divided society, it is not sufficient for historians to juxtapose the great contenders for racial superiority in the countryside. More than thirty years ago, C. W. De Kiewiet warned us that "the oppositeness of black and white, or the antithesis of European civilisation and tribal culture is not a sufficient clue to the relations between black men and white men."⁷² The important though elusive role that class plays in mediating everyday South African life must be understood by those who seek to uncover its troubled past and understand its tormented future—an exercise that always seems to make for an optimism of the heart but a pessimism of the mind.

⁷² C. W. De Kiewiet, *A History of South Africa* (Oxford, 1957), 179.

Reviews of Books

GENERAL

MICHAEL STANFORD. *The Nature of Historical Knowledge*. New York: Basil Blackwell. 1987. Pp. vii, 196. Cloth \$45.00, paper \$14.95.

Historical epistemology is an esoteric subdivision of the historical discipline. Most historians pay little attention to it, regarding it as not only abstruse but largely irrelevant. They do what they do without too much reflection about the philosophical assumptions behind what they are doing. For them creativity is more important than analysis; indeed, many fear that the latter may inhibit the former. Yet this resolute pragmatism does not seem to affect the quality of their work. One can be a good historian without knowing much about the theory of history, just as one can be a good artist without knowing much about the theory of art. "Theory is all gray," according to Goethe's Mephistopheles, "and the golden tree of life is green."

Occasionally some work examining the epistemological foundations of historical learning attracts wider attention. E. H. Carr's *What Is History?* (1962) and Isaiah Berlin's long article "History and Theory: The Concept of Scientific History" (1960–61) are good examples. R. G. Collingwood's near-classic *The Idea of History* (1946) is another. But these are rare exceptions. Historical scholars remain as a rule indifferent to the philosophy of history, so that most of what is written about it comes from philosophers rather than historians.

Michael Stanford's *Nature of Historical Knowledge* is not likely to change this situation. It is a clear, thoughtful, informative, and provocative analysis of what a historian does when he or she writes history. But it is highly theoretical and recondite. This is historical analysis in the strict sense of the term, examining the intellectual process whereby the historian imposes on an amorphous collection of facts an explicative design that transforms it into a work of history. Although this book is not for the casual or timid reader, it offers a sharp, perceptive analysis of what the historical scholars try to do and how they try to do it.

The author examines first the nature of what he calls "structures," those patterns by which historians seek to give meaning to their materials. He then looks at the events of the past as the constituent elements of the

historical field. These events in turn become transformed into "evidence," which leads to "construction," that is, a fusion of mind with data making possible a work of historical scholarship.

So far the book deals primarily with the interaction between scholars and their data. The second half examines how they go about communicating their findings or interpretations. First of all, there is the transition from "construction," which exists only in their imagination, to historiography, the actual writing of history. Then comes the effect of historiography on the mind of the public, that is, the communication of historical knowledge and its influence on collective perceptions of the significance of the past. The final chapters look at the way in which the writing of history, by shaping the public mind, can lead to action in history and thereby to the making of history.

Stanford concludes with a ringing profession of faith. "History consists of what men and women do and suffer in a four-dimensional world, time being the fourth dimension. The material of history is their actions, which are real and of intrinsic—even transcendent—importance. The structure of history is the relationships among human beings. But men and women themselves somehow escape being totally enclosed in the world of four dimensions. One wall of their dwelling is open to infinity. We are greater than history" (p. 189). Those who like to ponder the philosophical underpinnings of historical knowledge will find the book stimulating and illuminating. As for the others, it will be rough going, I'm afraid.

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MASSIMO MASTROGREGORI. *Il genio dello storico: Le considerazioni sulla storia di Marc Bloch e Lucien Febvre e la tradizione metodologica francese*. Naples: Edizioni Scientifiche Italiane. 1987. Pp. 274. L. 28,000.

As founders of the Annales school, Marc Bloch and Lucien Febvre enjoy near-legendary status among historians. Though both were wary of abstract theorizing, each left a corpus of more or less theoretical statements that, Massimo Mastrogregori feels, helps us under-

stand their fruitful but problematic legacy. His account of their unsystematic theorizing is notable because he is well versed not only in the French historiographical tradition since Charles-Victor Langlois and Charles Seignobos but also in the markedly different way of thinking about historiography that Benedetto Croce, especially, fostered in Italy. Indeed, Crocean categories inform Mastrogregori's criticisms, and his Italian angle enables him to pinpoint certain limiting, even provincial, features of the French tradition.

Mastrogregori fully appreciates, however, the enduring significance of Bloch and Febvre; he simply seeks the dialogue that should enable us to place their complex historiographical legacy in better perspective.

Mastrogregori's account turns on positivism, which continued to shape the French historiographical tradition even as reformers such as Henri Berr pointed beyond political narrative. The author repeatedly deplores the continuing French assumption that the historian selflessly copies some past object, an assumption that precludes attention to the necessarily active role of the inquirer-interpreter, whose concerns establish criteria of significance within the infinite field open to historical questioning. To be sure, Bloch and Febvre sought to depart from positivism—in their antideterminism, most notably, but also in their quest for historical concreteness, beyond any theoretical framework. By changing "history" from a naturalistic object into an apparently limitless field, open to inquiry on any level, their accents made possible the expansion of the historiographical focus long credited to the *Annales* tradition. But Bloch and Febvre bequeathed an ambiguous legacy precisely because each continued to think, in an essentially positivist way, in terms of "classification," with the historian simply discovering the correct arrangement of facts. For Febvre, especially, to open the field of inquiry was to invite total history, and this, Mastrogregori charges, results in historiographical leveling and flabbiness. He implies that attention to the active, organizing role of the historian becomes ever more necessary as the range for historical questioning expands. Otherwise, the initially welcome expansion of focus produces a blurring of focus and growing uncertainty about the cultural import of historical understanding.

Mastrogregori's fundamental criticism is important, but, because the deficiency at issue runs throughout the French tradition, that criticism is made with somewhat wearying frequency. And to readers unfamiliar with the Italian tradition, his criticisms may appear altogether too peremptory—even too ad hoc and schematic—to be convincing. Certainly he offers no systematic statement of the "active," postpositivist conception of historical inquiry that seems to underlie his criticisms. It becomes clear that his alternative rests on historical truth as opposed to the rhetorical, prefiguring that Hayden White has emphasized, but the reader can only infer what the generation and use of historical truth might involve.

Mastrogregori's treatment is thus by turns confi-

dently critical and modestly tentative, apparently because he feels, plausibly enough, that the achievements of his two protagonists, and the difficulties they have bequeathed to us, are bound together in ways that cannot yet be sorted out definitively. In any case, his appreciation for the positive side lends credibility to his criticisms, which suffice at least to suggest that important ambiguities, even superficialities, passed from the earlier French tradition via Bloch and Febvre into the *Annales* school. This balanced account invites further critical reflection about the French role in producing the current state of historiography, at once so promising and so problematic.

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COLIN RENFREW. *Archaeology and Language: The Puzzle of Indo-European Origins*. New York: Cambridge University Press. 1987. Pp. xiv, 346. \$29.95.

Archaeology in the last twenty years has been divorced from linguistics and mythological research. The appearance of this study, written by a prominent archaeologist, seemed to signal a renewal of interest in bridging archaeology with Indo-European studies. Unfortunately, Colin Renfrew did not correlate the results of Indo-European linguistics and comparative Indo-European mythology with those of archaeology.

The author's survey of the history of Indo-European studies is useful. It is not, however, a presentation of current problems of linguistic research. A number of leading Indo-Europeanists (James Mallory, Robert Coleman, A. Richard Diebold, Wolfgang Meid, Edgar Polomé, Eric Hamp, and others) have already stated in their reviews that Renfrew failed to account for the linguistic facts as we have them. The comparative mythological research essential for the understanding of proto-Indo-European society and ideology is entirely neglected. The archaeological part of the book deals with unsubstantiated hypotheses that will be accepted only by those who are not familiar with Indo-European studies and Eastern European archaeology.

Renfrew's solution to the problem of the Indo-European origin is one of the simplest proposed so far: the earliest agriculturalists of the Old World were proto-Indo-European speakers. Around 7000 B.C. this language must have been spoken in Anatolia and then migrated, around 6000 B.C., to southeastern Europe and, further, from southeastern to northwestern Europe and from Greece across the Mediterranean to Iberia. Those Indo-European-speaking farmers, through "demographic processes," assimilated or displaced the indigenous Mesolithic populations. The archaeological data show, however, that in most areas of Europe the introduction of agriculture was not the result of the movement or geographic displacement of people. Furthermore, the invention of farming involved a very large area and a number of distinct

culture groups. The first agriculturalists of Europe continued religious practices, symbols, and art from the Upper Paleolithic–Mesolithic. For this reason alone it would be impossible to see Indo-European traits in the earliest agricultural populations. According to studies of linguistics, paleontology, and comparative Indo-European mythology, the proto-Indo-European speakers must have been breeders of cattle, sheep, and, later, horses but rather poor farmers, acquainted with only a few kinds of cereal but in possession of ploughs. They were pastoral and mobile, equipped with wheels and vehicles. They were patrilineal and patriarchal, and they worshipped a sky-oriented pantheon of male gods (all male gods were horse-riding and equipped with weapons). The horse was from the very beginning of its domestication a sacred animal and its sacrifice a proto-Indo-European practice.

None of these features matches the ideology of social structure of the Neolithic populations, be they Europeans or Indians. The peaceful farmers were goddess worshippers. Their great temples, frescoes, sculptures, painted pottery, colorful textiles, and tombs as wombs have nothing to do with the Indo-European traditions focused on building fortifications, not temples, and on armament, not art. The tombs of Indo-Europeans were not wombs but imitations of tents or rectangular houses in which they believed they would continue the same kind of life in the same social class—king as king, warrior as warrior, and so forth.

To reconcile the fact of the existence of south Russian steppe pastoralists (who would be very difficult to prove not Indo-European speakers), Renfrew made them derive from the Cucuteni (Tripolye) culture in Moldavia and the western Ukraine of the fourth millennium B.C., whence they supposedly dispersed to the east. The Cucuteni culture was one of the highest civilizations in Europe of the Copper Age with all of the features of a gynocentric culture, continuous from the Neolithic period. According to the data of archaeology, south Russian steppe pastoralists—the Kurgan culture—derived from the Volga Neolithic culture of the seventh and sixth millennia B.C. When they mounted the horse (probably not later than the end of the sixth millennium B.C.), they became mobile and pastoral, and they spread west, south, and east. Furthermore, to explain how the Indo-Europeans reached India, Renfrew put the first agriculturalists in India and made the Indus (Harapa) civilization Indo-European.

It is important for the question of Indo-European origins to follow the development of a Neolithic culture in the middle and lower Volga basin. There the Neolithic age started almost at the same time as it did in southeastern Europe, but it took a very different course. The emphasis was on animal (cattle, sheep, and, from at least 5000 B.C., horse) domestication and foraging, not on cereals and horticulture as it was in Europe. The culture of the Volga basin was male-dominant, and it matches the picture reconstructed by scholars of Indo-European linguistics. In the early fifth

millennium, the Volga Neolithic culture spread between the Don in the west, the Caucasus in the south, and Kazakhstan in the east. By 4500 B.C., the mounted warriors equipped with arrows, daggers, spears, and shields became a force at the border of the agricultural Old European civilization. Soon thereafter they appeared in the lower Dnieper and lower Dniester region and, via the lower Danube valley, entered into the heart of Old Europe, causing the disintegration of its most civilized parts in Romania, Bulgaria, and Yugoslavia. To see, as Renfrew does, a west-east movement of the Kurgan people out of a gynocentric Europe is a twisting of archaeological facts and a distortion of European prehistory. Renfrew even diminishes the importance of the horse by placing the time of its domestication nearly 3000 years later (according to him, the horse became important only in the middle of the third millennium B.C.). The horse played an enormous role in the Indo-Europeanization of Europe. One wonders why the author is so disinterested in Eastern European prehistory. In proposing “solutions” to the problem of the origin of the Indo-Europeans, it is essential to have an insight into the ideology and social structure of the people described.

European culture is a *mélange* of two very different cultures, a *mélange* reflected by language, mythology, and social structure. A non-Indo-European substratum and an Indo-European superstratum are recognizable. For instance, the agricultural terminology—names for cereals and domestic animals, fruits and vegetables, and goddesses—is mainly non-Indo-European. Anatolia and Greece (a “proto-Indo-European area,” according to Renfrew) are full of non-Indo-European cultural elements. These areas contribute very little to the reconstruction of Indo-European religion and myth. Instead, the two strata are opposed, and the Old European mythic substratum prevails. The Indo-European culture as we know it could not have developed out of the rich, earth-bound, fully agricultural, gynocentric civilization of Europe. In my view, not farming but androcracy, the horse, and the dagger led Indo-Europeans to world dominance. The Indo-Europeanization of Europe was a traumatic process that continued for about two thousand years, but even then Europe was never completely Indo-Europeanized. Islands of non-Indo-European culture remained throughout the Bronze Age and later: the Cycladic islands, Thera, Crete (who would say that the Minoan culture of 3000–1500 B.C. was Indo-European?), and many areas along the Danube in Yugoslavia and Romania. Such cultural islands are also known historically: the Picts in Scotland, Etruscans in central Italy, Iberians, and Basques. The Basques have survived to this day.

This book appears to be the product of haste and the result of an obsession for creating new hypotheses without adequate foundation. It is a pity that a professor of Cambridge University was trapped by the complexities of Indo-European studies and did not find the way out. The task of bringing together the results of

Indo-European linguistics, mythology, and archaeology needs another kind of methodology than that used by Renfrew. Let us hope that the many reviews by Indo-European scholars dismissing Renfrew's "solution" of the Indo-European origins will widen the horizons of archaeologists and will stimulate highly necessary interdisciplinary studies, since current archaeology is predominantly economic archaeology, a tree stump with no leaves.

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BRUCE M. METZGER. *The Canon of the New Testament: Its Origin, Development, and Significance*. New York: Clarendon Press of Oxford University Press. 1987. Pp. x, 326. \$55.00.

This weighty and useful volume is the third in a series by Bruce M. Metzger; the first two dealt with the text of the New Testament and early versions of it. This book opens with a judicious critical analysis of scholarly writings on the subject of the canon prior to and during the twentieth century. The factors that led to the formation of the canon are described, beginning with the Apostolic Fathers. Especially illuminating are the analyses of the import of movements within the early church, as well as external challenges to the church, for the question of the canon of the New Testament. Movements within the church include Gnosticism, with its claim to esoteric knowledge that supplements and basically alters the outlook of the writers of the New Testament; the attempt by Marcion to sever the connection with Judaism by discarding the Old Testament and reducing the authoritative Christian writings to an expurgated edition of Luke and the letters of Paul; and Montanism, which claimed to supplement the New Testament by revelatory insights and discourses. The persecution of the church, especially under Diocletian, forced the Christian leaders to make decisions as to what the Scriptures were that the emperor insisted be destroyed. The development of the canon is then traced in the eastern regions (Syria, Asia Minor, Greece, and Egypt) and in the West (Rome, Gaul, and North Africa). At the same time that these moves toward specification of the New Testament canon were taking place, certain segments of the church were producing and promulgating additional writings, such as the apocryphal gospels, acts, and apocalypses that claimed to be elaborations of the writings that formed the core of the New Testament. Decisions had to be made about the acceptability and authority of those writings.

Even as efforts were made in both East and West to define the list of books to be considered authoritative, there were certain books from the period of the Apostolic Fathers that appeared frequently on the fringe of the canon, such as the Didache, 1 and 2 Clement, and the Epistle of Barnabas. The earliest known lists of

New Testament books include the so-called Muratorian canon from late second-century Rome and that of Eusebius in the early fourth century. A major factor in the East was the canonical letter of Athanasius of Alexandria in 367, in which Athanasius listed the twenty-seven books of what is now regarded as the New Testament. The great value of Metzger's presentation is that he has not only traced the development of these canonical formulations but also included the actual lists in an appendix to the volume. Throughout this debate in the early centuries of the church, the recurrent points of contention were the Catholic Epistles and the Revelation of John. On the gospels and the letters of Paul, there was nearly universal agreement. The disputes about the Catholic Epistles continued through the Renaissance and Reformation. The issues were whether the Rule of Faith determined the extent of the canon or was shaped by it and what constituted apostolicity and authority of Scripture. These issues continue to the present day, as Metzger neatly sorts them out: which form of the text is canonical? Is the canon open or closed—for example, could a newly found document receive canonical status? Is the canon a collection of authoritative books or an authoritative collection of books? The author's conclusion is that the church came to recognize the inherent authority of certain writings. The proliferation of writings in subsequent centuries and the recent discovery of ancient documents serve to increase awareness of the uniqueness of those documents that the church came to regard as authoritative. This book is of great value, not only as a careful survey of the issues historically but also as a contribution to the current scene, in which certain biblical scholars are seeking to reduce or to expand the canon. This issue is not a dusty relic of the past.

HOWARD CLARK KEE
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MOSHE IDEL. *Kabbalah: New Perspectives*. New Haven: Yale University Press. 1988. Pp. xx, 419. \$40.00.

"This study is based upon the assumption that there are two major trends in Kabbalah" (p. xi). With this opening sentence, which plays on the title of Gershom Scholem's classic study in the field, Moshe Idel launches into his revision of the master's work, creating in the process another masterpiece of scholarship and insight.

The core of Idel's critique of Scholem contains two ideas. First, Idel acknowledges, but rejects as limited, the historical-textual approach of Scholem. In turn, Idel proposes a phenomenological analysis of Jewish mystical texts (especially, pp. xii, 23). Second, Idel questions strongly Scholem's assumptions that historical events form mystical tendencies and, hence, that mystical phenomena can be put in sequence by historical causation. In turn, Idel relies on the inner logic of mystical tendencies to explain their development (especially, pp. 213ff., 253ff., 264–67).

Specifically, Idel rejects Scholem's view of the flowering of kabbala as a "gnostification." Rather, Idel maintains, the flowering of kabbala is a response to Maimonides' denial of mysticism and a product of the concentration of a large number of scholars and mystics in one area for a short period of time. This "innovative Kabbala" (p. 212) lasted only a short while and was followed by a period of systematization and later by a period in which the kabbala was viewed as an exoteric theological text to be decoded (pp. 30–32, 210–18, 250–56). Idel also rejects Scholem's view of the Lurianic kabbala as a response to the expulsion of the Jews from Spain. Idel points out that Luria does not mention the expulsion and that there is no compelling psychological reason to link those two phenomena (pp. 264–66). Idel rejects Scholem's view that the false messiah, Shabbetai Sevi, was an outgrowth of Lurianic kabbala. He points out that Shabbetai Sevi was not a Lurianic kabbalist, that Lurianic kabbala was not widespread in Sevi's time, that Sabbatian kabbala itself was much too arcane for the masses who participated in the movement, and that the messianism of Sevi and Luria was very different (pp. 257–60, 266). And Idel rejects Scholem's view that Hasidism and the Jewish enlightenment were responses to Shabbetai Sevi and Lurianic kabbala. It just is not so; there is no historical evidence (pp. 256–60, 264–67).

Idel also rejects Scholem's view of symbolism and allegory (pp. 218–22), his assertion of the lack of *unio mystica* in Judaism (p. 59ff.), and his tendency to ignore mystical techniques (p. 74ff). Idel is certainly correct in all of these criticisms. He should, however, have clearly labeled Scholem as a rationalist trying to contain the irrational, as a colonial rationalist. He also should have traced Scholem's linking of mystical and historical phenomena to Scholem's experience of the Holocaust and Zionism.

On the matter of the phenomenological analysis of Jewish mysticism, Idel is brilliant. He develops a typology of ecstatic-unitive and theosophical-theurgical mysticism (p. xviii). The ecstatic-unitive type is anthropocentric and largely anomic in its inspiration and techniques (pp. xi, xv, chap. 5). It is individualistic and contains a hermeneutic that "monadizes" each letter (pp. 208–09). It revolves around a passive experience in which the divine invades and possesses the mystic. This experience is definitely unitive (pp. 210, 235, 244–45).

The theosophical-theurgical type is theocentric and largely nomic in its rootedness in texts and Jewish ritual practice (pp. xi, xv). It is communal and has a hermeneutic rooted in symbol (pp. 208–09). It revolves around an active experience in which the mystic invokes divine and human energies to maintain cosmic and divine harmony. This experience is definitely imitative of the divine, theurgical (pp. 210, 232–33, 268).

This phenomenological typology will prove to be Idel's greatest contribution to the field. There are other studies: on *unio mystica* (chap. 4), four mystical tech-

niques (chap. 5), the nomiac-anomiac typology (also chap. 5), the different types of symbolism and allegory (pp. 218–22), and so forth. And there are other books by Idel with other detailed studies on various topics.

The arguments are laid out with massive evidence, embodied in 118 pages of notes with Hebrew texts where necessary and 18 pages of indexes. Sometimes the material is overwhelming, the categories not drawn clearly, diagrams missing (especially, p. 108); often summaries are lacking. But the sheer scholarship and insight of this work are remarkable. Jewish mysticism can no longer be studied, and Scholem can no longer be read, without this book.

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MICHAEL A. MEYER. *Response to Modernity: A History of the Reform Movement in Judaism*. (Studies in Jewish History.) New York: Oxford University Press. 1988. Pp. xvi, 494. \$39.95.

No topic in the history of the Jewish people in recent times has been so difficult, so fraught with nervous controversy, and yet of such fundamental historical importance as the painful and extended process by which many Jews have sought—while others have refused—to come to terms with modernity. The difficulty of this process has derived partly from the ever-growing social and political complexity of the Jewish condition as it has evolved in the course of the last two centuries or so and the ever more striking variations in the conditions that discrete Jewish communities have needed to confront. But the problem of modernity has also had much to do with the seemingly intractable character of the theological and ideological issues that arose for Jewry *pari passu* with social change. There is thus a sense in which no general characterization and analysis of the Jewish "response to modernity" as a whole may really be possible. It may be that no attempt to grasp this "response" in all of its variety over time and across the full geographic range even of the major components of the Jewish world, from North America to Yemen and from Morocco to Lithuania, can succeed. For there was no single, generally pervasive response throughout Jewry that the social historian and the historian of ideas can effectively pin down, analyze, and trace to its sources. If the face contemporary Jewry presents to the world as the process approaches its terminal point is, in fact, one of unprecedented incoherence, that visage owes much to the fact that Jewish emancipation—one of the fundamental conditions on which the "modernization" of the Jews has hinged—has itself been slow and uncertain and subject to brutal reversal even in lands where it actually occurred. (In many parts of the world there was, of course, no emancipation at all, and serious public debate on it, let alone pressure to institute it, was unknown, if not unthinkable.) Yet the present confusion may have had even more to do with the equally central circumstance

that what modernity repeatedly and specifically called into question in Jewish life were precisely the common practices and beliefs of the Jews world-wide, part and parcel of their identity and structure as a coherent nation.

Michael Meyer's new book does not attempt to deal with the full range of issues that may be implied by its title. Ostensibly, as the subtitle suggests, his work is a more modest endeavour, an account of one strand in the tangle of the larger subject. The book is nonetheless welcome. The movement for the religious reform of Judaism is *ex definitio* of fairly specific thematic limitations. In practice, at least until very recently, it was to all intents and purposes a movement limited geographically to Central and Western Europe and the Americas. Nevertheless, the case of Reform Judaism is extraordinarily instructive not only for our understanding of contemporary Jewry as it stands today, now that so little is left of Eastern European Jewry and of the Jewish communities of the Islamic world, but especially for our general grasp of the acute problems presented to those Jews who, from the first, were both confronted by, and yet most intent on meeting, the challenges posed by the grant of relatively unimpeded entry into the modernity that seemed on offer. Surprisingly, the last serious attempt at an overall view of the subject, David Philipson's *The Reform Movement in Judaism*, was originally published no less than eight decades ago. Meyer's treatment supersedes Philipson's book not simply because it is inevitably more up-to-date but also because Meyer has succeeded admirably in placing religious reform, as it evolved and was practiced in the nineteenth century, notably in Germany, Great Britain, and the United States, within the broad social and intellectual context of the times and, in a limited way, in the political context as well.

What emerges very clearly from Meyer's account is that, for all the scorn and contumely heaped on the reformers by the orthodox, who have invariably seen themselves (and still see themselves) as speaking for mainstream Jewry and a pristine Judaism, Reform Judaism was ultimately a less radical movement than the Jewish Enlightenment of autonomism or Zionism or Bundism or any of the other social, philosophical, and political movements for change in Jewry, chiefly in Europe, that arose in the course of the last century and tended to gather greatest strength at the beginning of the twentieth. In retrospect, Reform Judaism may even be judged to have been less threatening to orthodoxy itself than was (and is) often supposed. Of course, on the face of things, this notion is paradoxical. Reform Judaism not only attacked religious orthodoxy at its crucial pressure points of law and ritual but also proposed to deal with the increasingly vexed question of Judaism as a structure entailing both religious and national identity by putting all possible emphasis on religious identity, while discounting, or even wholly denying, national ones. But, whereas the other movements for social change in Jewry were essentially, or even explicitly, secular and nationalist, Reform Juda-

ism sought to restate and recast—but not do away with—the religious belief and practice of the Jews. And that being the case, which is to say, given that Reform Judaism's concerns were with what undoubtedly did pertain to the realm of the spiritual, there was at least some common ground with orthodoxy, even if, on the orthodox side, it was rare for this fact to be admitted. The greatest threat to orthodoxy (as the wisest of the orthodox leaders understood only too well) stemmed from the out-and-out secularists who offered what seemed to be a much more complete, by no means less attractive, and still more modern alternative to ancient practice.

It remains, of course, that the fundamental difficulty for the religious reformers, as Meyer makes abundantly plain, has always been first and foremost the very notion of reform or change *per se*. Can there be change in Judaism? That was the central question from the first, a question no less weighty than that of the content and thrust and likely consequences of the changes envisaged. Moses Mendelssohn himself, the distant godfather of the movement, did not really think so. And doubt, undercutting the reformers' self-confidence, has persisted. In the past, all was held together by a form of case law, by attachment to the system itself, by respect for its foremost authorities and protagonists, and, not least, by fear lest damage to the delicate fabric of consensus bring about the collapse of the entire structure. In the main, it was precisely from this, namely from the weight of law and custom elaborated over many centuries, that the reformers wished to escape—an escape that was doubly shameful to the orthodox because in practice it signified an escape toward the forbidden and anathematized world of the Gentiles. So choosing, the reformers were judged to have damaged the fabric of consensus and respect, possibly beyond repair. Meyer does not deal in detail with these, the larger consequences for Jewish life and society of the rise of Reform Judaism, which is something of a pity, but the subject looms inescapably.

Another theme in this book that emerges with great clarity is the special energy with which the reform movement was invested on its arrival and establishment in the United States. In what many newly arrived European Jews quite rightly perceived as the "liberal, seductive atmosphere of America" and in response to the distinct danger that the Jewish "inheritance could evaporate" (p. 260), the reform movement in the United States became *inter alia* nothing less than a vast effort—in many cases a last-ditch attempt—to stave off "evaporation." The result was the offering and adoption of forms of Judaism and private and communal behavior that were all too plainly products of the pressure and pull of the non-Jewish world and what seemed to many to be the virtually irreversible inroads of acculturation. But, given its premises, American reform, like German reform before it, had no choice but to search for an acceptable and, above all, attractive synthesis of old and new, of foreign and native. In any event, so doing, by and large, American reform pro-

ceeded to show itself more thoroughgoing, more inventive, less self-conscious, and, in many ways, more in tune with the society at large in which it was placed than ever were its European counterparts. And its course is not yet run.

All told, this work is a most valuable monograph—authoritative, sensible, meticulous in its use of resources, and clearly written. It promises to serve as the standard work on the subject for years to come.

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ANDRÉ BURGUIÈRE *et al.*, editors. *Histoire de la famille*. Volume 1, *Mondes lointains, mondes anciens*; volume 2, *Le Choc de la modernité*. Foreword by CLAUDE LÉVI-STRAUSS. Paris: Armand Colin. 1986. Pp. 639; 559.

The reader of these two volumes will learn far more than simply the history of the family. As Claude Lévi-Strauss points out in his introduction, family analysis has horizontal and vertical dimensions requiring the skills of both the historian and the anthropologist-ethnologist. Hence, the volumes are an excellent introduction to these two modes of thought about familial matters. Furthermore, we have here a heroic attempt at global, implicitly comparative, family history, yielding, in some cases for the first time in a Western language, outstanding synthetic accounts of the subject in extinct and living, non-Western and ancient societies. Although the craft of family history now includes a number of scholars, some represented in these volumes, who practice historical anthropology (or anthropological history), none possesses the encyclopedic knowledge required to treat the history of the family at the world level. The logical decision of the editors, therefore, was to assemble a group of specialists of varied national and disciplinary backgrounds (though most are French), who had some sympathy with, if not actual leanings toward, the tripartite—historical, anthropological, comparative—goals of the project. The result is commendable: an intelligent state-of-the-art survey meant for an audience of nonspecialists, all the more satisfying because of the wealth of illustrative material.

In volume 1, Claude Lévi-Strauss contributes the aforementioned introduction and Françoise Zonabend a lengthy essay on the ethnological concepts and techniques for dealing with kinship and the family, both horizontally and vertically. The culturally specific part of this volume contains three fairly evenly balanced sections on the family in the ancient world, in medieval Europe, and in non-European cultures. In the first section, Claude Masset covers the prehistory of the family, dwelling appropriately on the difficulties of basing credible generalizations on archaeological evidence, and Jean-Jacques Glessner deals with the somewhat more rewarding records of ancient Mesopotamia, which permit credible observations not only about

genealogy but also about male and female roles and family ritual. Annie Forgeau's essay treats ancient Egypt and demonstrates how increasingly reliable records permit analysis of such matters as rules of alliance, multiple marriage, and the composition of domestic groups. Giulia Sissa describes the ancient Greeks in their city-states, the central concept of the *oikia*, and the merging of private and public dimensions of family life. Yan Thomas and Aline Rousselle each contribute a chapter on Rome. Thomas deals primarily with the republican period and the questions of patriarchal power, inheritance, and marriage, and Rousselle describes the empire and discusses, among other things, education, the lineage ethic, and the modifications of family life introduced by Christianity. In the medieval section, a short preface by Georges Duby lays out the problems of family analysis in the "dark ages," and the first essay on "barbarian" Europe by Pierre Guichard and Jean-Pierre Cuvillier treats the discrepancies between historical facts and the classic formulations of the Germanic *Urfamilie* and *Sippe* and deals with the penetration of Roman family law among the merging Germanic and non-Germanic peoples. An essay on the Carolingian period from the eighth to the tenth centuries by Pierre Toubert describes demographic impacts and the domestic group, and another on the period of classical feudalism from the eleventh to the thirteenth centuries by Robert Foussier discusses, among many other things, the consequences of marriage proscriptions. The essay by Henri Brec on medieval Europe after the rise of towns (from the thirteenth to the fifteenth centuries) considers the idea of the patriarchal household, the couple, concubinage, and patrimony. Concluding the European section, an exceptionally informative essay on the Byzantine empire by Evelyne Patlagean deals with the interpenetration of church, state, and the family domain and discusses the Byzantine sources available for studies of the topic. In the last section, which deals with the history of family life in the largest non-European civilizations, Michel Cartier discusses the evolution of lineage kinship and family values in China; Patrick Beillevaire centers his survey of Japanese family history on the rise of the *ie* and analyzes Japanese kinship; Roland Lardinois explains the problems of sources for family history of India and the importance of patrilineality; and Thierry Bianquis contributes an excellent survey of the historical family in Islamic civilization.

Volume 2 describes the diversity of contemporary family forms brought about by various kinds of modernizing change. After an introduction by Jack Goody reiterating the need for continuing horizontal and vertical analysis, André Burguière and François Lebrun together contribute two essays on the last several centuries in Europe, where population change, geographical and religious diversity, and the emergence of the couple have created a very complicated modernity. A somewhat less historical review of the non-European world is then pursued under the rubrics of the colonial experience and subsequent national development.

Here Carmen Bernand and Serge Gruzinski describe the familial past and present of central and south Americans; Michel Cartier analyzes the impact on the Chinese family of successive twentieth-century communist and noncommunist revolutions; Patrick Beillevaire looks at the importance of the modern Japanese family for state-building and the development of industrial enterprise; Roland Lardinois explores the emergence of contemporary family law in India; Jean-Pierre Dozon reviews the connections between family change and underdevelopment in Africa; and Philippe Farues, in perhaps the most statistically minded chapter of all, examines the diversity of contemporary family life in the Arab world. The final section in this volume returns to the Western world and looks not only at the impact on Europe of the Industrial Revolution (Martine Segalen) but also at some representative "modern" family systems, such as the American (Hervé Varenne), those of "socialist" Europe (Basile Kerblay), the Scandinavian (David Gaunt and Louise Gaunt), and the French (Martine Segalen and Françoise Zonabend). Understandably, these chapters highlight subjects such as the growing significance of government, family planning, changes in the status and rights of women, and shifting family roles. In an afterword, the editors of the two volumes offer a short critique of conventional evolutionary theories of the family and reject the proposition that the family as an important social institution is heading toward extinction.

Pitfalls abound in the launching of collective enterprises such as this; not the least threatening is the natural conflict between the anthropological bias toward synchronic analysis and the imperatives of diachronic description of both short- and long-term historical change. Moreover, world-wide coverage of the subject, a strength of these volumes, contains its own dangers when the number of authors is as large as it is in this case. An analogous study of world-wide family change in the recent past, William Goode's *World Revolution and Family Patterns* (1963), avoided potential problems precisely because it was written by a single author. Fortunately, the editors have guided this collection with a firm hand, helped perhaps by the shared conceptional equipment of the authors, and, thus, centrifugal tendencies have been kept to a minimum. A glossary defines specialized anthropological terminology, and a forty-two-page bibliography represents the literature of the family history field very well.

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G. ROBINA QUALE. *A History of Marriage Systems*. (Contributions in Family Studies, number 13.) New York: Greenwood. 1988. Pp. xii, 399. \$45.00.

This book represents an exceptionally ambitious undertaking, based on wide reading conveyed both in the text and in a useful bibliography. The purpose is no less than to chart the major marriage systems that have

appeared in human societies, across the globe, from the era of hunting and gathering to the present. Primary focus rests on marriage laws and basic marriage types, but there are excursions at points into birth rates and other matters (although emotional styles are given distressingly short shrift). The explanatory framework leans heavily on material systems, as marriage patterns are seen as responding to economic constraints and opportunities, but there is some consideration of culture and occasionally some sense that marriage systems might be a cause, in history, as well as an effect.

The book achieves its primary purpose as a reference work, and judicious use of the index allows fascinating cross-cultural exploration of topics such as bride price or inheritance systems. In addition, the treatment offers useful fodder for world history and comparative courses that seek to extend to family history; there is solid material for many lectures in these carefully researched pages.

There is also a larger analytical framework. G. Robina Quale is concerned not only with major marriage typologies—where she treads a solid but well-worn path—but also with a general periodization schema. She distinguishes among hunting and gathering, early agricultural, commercializing agricultural, and industrializing cases, with interstitial discussions at key points of transitions from one type to the other. This schema obviously plays to the dominant economic systems framework; it also allows a wide sweep across Asia, Europe, the Americas, and Polynesia, although there is a tendency to slight India and Africa. A conclusion also raises yet another analytical vantage point in arguing that certain societies may see extremes in marriage arrangements or contexts played out to virtually self-defeating lengths—state power (her example, a shaky one given what is now known about totalitarian limitations, is Nazi Germany), family alliance (the Hindu example), and individualism (contemporary United States). More fruitful, so the implication runs, are more nuanced approaches to the marriage partnership that push no one ingredient too hard.

These analytical frameworks are interesting, but they do not convey a vigorous sense of debate. The larger chronological chunks encourage some comparative analysis, but it has none of the bite of Jack Goody's work (which is indeed often cited). Specific comparative data are of course presented, but, when Quale moves beyond the level of summaries of attributes, she tends to generalize. And, despite the conclusion, there is no clear sense, throughout the book, of marshaling evidence toward an ultimate argument. Indeed, more of the book is presented in terms of summaries of more specialized work, one piece at a time, than is fully desirable even in a survey—a pattern encouraged by the lack of footnotes, which compels distracting textual references, at times on a notecard by notecard basis.

The book also misses a good intermediate sense of history, which may limit its classroom utility aside from its service as a reference. Within the broad chronological chunks, not a great deal of change is traced;

juxtapositions of the thirteenth century and the nineteenth century are common. Shifts in family arrangements from the late Middle Ages to the early modern period in the West are handled in greater detail, but, even for the modern West, and certainly for most of the rest of the world, actual case studies of transition from one marriage style to another are missing.

Although this is not a compelling treatment in terms of overall thesis or nuanced historical sense, its ambitious competence will serve many readers well. Not only factual coverage but a host of particular insights command respect. Even the chronology, although bulky, may advance a much-needed process of bringing advances in the social history of the family into an appropriate global focus.

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E. L. JONES. *Growth Recurring: Economic Change in World History*. New York: Clarendon Press of Oxford University Press. 1988. Pp. xii, 247. \$49.95.

E. L. Jones likes to address big questions. His little book *The European Miracle* (1981) anatomized what he then took to be unique circumstances that allowed Europeans, thanks to the Industrial Revolution, to outstrip the rest of the world in modern times. But he was not entirely satisfied with his own arguments and in the book under review suggests that the European miracle was not so very miraculous after all, being only one of a number of instances in which intensive growth, that is, increase in per capita wealth, was achieved locally and for a limited period of time by peoples as diverse as ancient Athenians, Sung Chinese, and Tokugawa Japanese.

Seeking to understand these sporadic outbreaks of economic advance, Jones eventually decided that the strategy of his earlier book was intellectually unsatisfactory. The various circumstances conducive to growth that he had anatomized before now seemed to him more like adaptive consequences of growth or mere local peculiarities of custom or practice that had nothing essential to do with making growth happen. Instead, he came to the view that a propensity to growth-inducing behavior was endemic among all humans. "Some people in every large society have worked to invest, invent, innovate, and so improve their material circumstances so that recurrent moves in that direction by whole societies become historically probable" (p. 185).

The question then becomes: What holds growth back? Extensive economic growth was, Jones argues, pervasive. Civilized societies regularly tended to expand to new ground, and more intensive use of the landscape already occupied by complex societies also occurred steadily—but without observable increase in the wealth or comfort of the common people. By comparison, intensive growth, when improvements in productivity more than kept up with population

growth so that GNP per capita actually increased, was always rare. Nonetheless, the "take off" of eighteenth-century England was not unique, and efforts to explain intensive economic growth solely in terms of the Industrial Revolution in Great Britain now seem to Jones to be naively ethnocentric and myopic.

Instead, he suggests that the propensities favoring economic growth were countered by equally widespread propensities for what he calls "rent seeking," that is, getting something for nothing by seizing it and consuming the product of others' labor and ingenuity. Exposure to confiscatory rents, taxation, and plundering, Jones believes, ordinarily kept wealth-increasing propensities severely in check. What was needed, he suggests, was a political system in which public authority was effective enough to inhibit local strong men and ordinary brigands from checking growth by excessive plunder and rent taking but in which the sovereign authority was itself held in check by countervailing political-military organized force—and thus unable to skim off new wealth by confiscatory taxation.

This summary of Jones's argument is far too brief to do the book justice. He has supplemented his expertise in European economic history by reading widely in Asian history, ancient and modern, familiarizing himself with the record as presented by scholars using humanistic and linguistic vocabularies and then putting their data through the sieve of contemporary economic theory. The result is designed to challenge the prevailing paradigm of European economic history and will do so if Jones's fellow economic historians accept the proposition that there are forms of intensive growth other than those of the Industrial Revolution and frames of economic management other than that of Western European capitalism.

Jones's style is both delightful and lapidary. He strikes innumerable sparks with challenging assertions on almost every page. At the same time, he is epistemologically sophisticated and extremely self-conscious, recognizing how much the way one frames the question affects the answer. In sum, his new book is as thought-provoking as *The European Miracle*—not least where it contradicts its predecessor.

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CLAUDINE HERZLICH AND JANINE PIERRET. *Illness and Self in Society*. Translated by ELBORG FORSTER. Baltimore: Johns Hopkins University Press. 1987. Pp. xvi, 271. \$28.50.

This is a remarkable book. Alongside Arthur Kleinman's recent volume, *The Illness Narratives* (1988), it is likely to prove enormously useful as a guide to one of the hidden texts in the social studies of medicine: the personal experience of illness. Since Susan Sontag's *Illness as Metaphor* (1977), there has been a gradual accumulation of interest in the place of the patient in the evolution of medical care. Until quite recently,

however, the emphasis has remained on the patient's role within the wider social and political fabric, essentially as a cipher-like "player" in a social system, that is, medicine, that mirrored the larger picture but nonetheless sustained its own proprietary rules. Over the last ten years, we have slowly begun to see, in the work of Charles Rosenberg and others, an emerging historical perspective that describes the growth of medical institutions from the bottom up: a radical and most welcome departure from the triumphal (or occasionally antitriumphal) puff and circumstance that bloodlessly preceded it.

But the examination of the role of the sick has remained, until much more recently, just that: an analysis of a role, a mapping of individuals according to race, class, and economic circumstances onto the sprawling terrain of hospitals, diseases, and social expectations. The last remaining frontier was that of an inner space, the personal world of illness in which objective sickness or disease might be charted within the very different terrain of an individual life. The new generation of explorers of the inner history of disease asks some of the truly difficult questions about illness as a deeply felt experience. Those new cartographers of the inner terrain are now beginning to emerge, and Claudine Herzlich and Janine Pierret are in the vanguard.

Herzlich and Pierret's work began with empirical field studies during the early 1960s and moved only later to historical retrospect. The authors know that illness and good health are themselves important counters in the shifting equation of individual life stories. And they know that the experienced value of illness is something quite different from the social place of sickness and disease. In this book illness takes its place alongside other intangible cultural symbols and values, such as identity and dissolution, work and isolation, sin and punishment, engagement and self-absorption, penitence and fate. There is much here about fate. In a novel and insightful flourish, Herzlich and Pierret claim a key normative shift to have occurred between 1960 and 1980. Perhaps as part of the residue of 1968, they see a significant and precipitous fall-off in the general sense of the "fatedness" of illness and death, concomitant with a vast enhancement in individuals' sense of entitlement to medical care, sick leave, and, indeed, health itself.

Illness is also a cultural context that can be seen to shift over time. Part of the genius of Herzlich and Pierret is their willingness to blur our habitual disciplinary boundaries between history and "soft" social science. The authors move easily from examples drawn from early modern literature to their favorite era, the nineteenth century of Honoré Balzac, Victor Hugo, and Alphonse Daudet, down to the present, then back again. (Ironically, the book was completed in the early 1980s and published in the original French in 1984; hence, it ends just short of AIDS.)

The catholicity with which the authors select their examples on a chronological scale does not come with-

out a price. There is unquestionably a meandering and even circular quality to their essay. We are told at regular intervals that the decline of the romantic view of tuberculosis correlated closely with the tandem rise of professionalism and of the objectified value of health. We are reminded again and again about Daudet and his syphilis. Up to a point, a stronger editorial hand might have streamlined the work through tighter organization. But only up to a point, because this is ultimately neither a conventional work of social science nor of history. Indeed, what Herzlich and Pierret forego in terms of narrative, they regain in terms of an easy, comparative vigor, moving swiftly to and fro between past and present. Their volume should be read more as an extended meditation with both historical and contemporary examples than as either narrative or rigorous sociological analysis. And in that context it succeeds admirably.

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LEONARD A. SAGAN. *The Health of Nations: True Causes of Sickness and Well-Being*. New York: Basic Books. 1987. Pp. 233. \$19.95.

Epidemiologist and physician Leonard A. Sagan believes he has detected a recent "decline of health in the United States" (p. 4) as measured by life expectancy, infant mortality, morbidity, and health habits (that is, obesity, exercise, and drinking). Aside from the obvious reasons for distress about that situation, Sagan is troubled that the putative deterioration in health has coincided with a large increase in medical spending, from 4.4 percent of the GNP in 1950 to 11 percent in 1984. Is the U.S. health strategy—a strategy that stresses the detection and treatment of disease—the most effective way to go about prolonging life and enhancing health?

The argument Sagan makes about those issues is essentially a historical argument, albeit one not widely informed by historical reading. That is a shortcoming of some importance but, most of us would probably concede, one of less importance than the prospect of medical treatment by a historian who has not read much medicine. The point here will be to see whether the historical case Sagan makes in this book may have some compelling features anyway.

First, it is useful to ask whether there is something of a crisis in health among Americans, as Sagan claims. The United States lags behind many other developed countries in reducing infant mortality. But the U.S. death rate has declined since 1968, after remaining stable between 1955 and 1968, and life expectancy has increased, especially in the oldest age groups. Morbidity—the proportion of people reporting that their ordinary activities are limited by health problems—has increased markedly since 1957, when such information began to be gathered regularly. Morbidity has increased also in Britain, Japan, France, Canada, and

other countries where similar data are obtained through surveys. Health habits are more difficult to identify and interpret. Some observers, notably James Fries, have claimed that Americans are becoming much more attentive to health advice, an attentiveness they show by exercising. Sagan notes that the need to exercise, which is itself related to consuming more calories than are used without exercise, is rising. Even if the signs of an American crisis in health are less compelling than Sagan claims, it is still true that spending on health has increased far more rapidly than any measure of health status has improved and that death rates declined to a low level before the health build-up began.

The chief contribution of this book is an argument about the modern rise of life expectancy. Most authorities have attributed that rise to improved material conditions: public health reforms, better nutrition, and improved housing. Sagan questions that view, noticing some inconsistencies in it, and argues that the key to rising life expectancy lies in the individual's capacity to survive disease. He maintains that the case fatality rate of disease declined faster than the death rate in the late nineteenth and early twentieth centuries.

Resistance to disease improved more than the risk of exposure diminished, but Sagan questions whether overt or so-called hidden hunger (undernutrition as a secondary cause of death) played a large role in determining whether people fell sick in the decades before 1870–1900, when death rates began to fall. In place of a choice between the public health and nutrition explanations, Sagan offers a new argument. Standing conventional wisdom upside down, he claims that premodern society was stress-ridden, its anxieties caused by concern over meeting basic material needs and by contentiousness within society and the family. The case fatality rate and, somewhat later, the death rate declined because people faced less stress and because their immune systems were less often disabled or compromised by psychological stress. This argument stems from research undertaken in the last twenty years, which shows that stress plays a role both in separating those who recover from disease from those who do not and in the pace of recovery. What is unclear from that research, and from Sagan's book, is how large a role.

Another physician who was interested in these matters, Thomas McKeown, made his case that death rates declined because nutrition improved by raising doubts about other explanations and claiming the field for his own view. Sagan repeats that procedure, reporting little positive historical evidence and advancing some dubious claims. Nevertheless, his argument about stress and its role in disease demands scrutiny and a search for information that will tell us more about both the nature and the history of stress.

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NORRIS S. HETHERINGTON. *Science and Objectivity: Episodes in the History of Astronomy*. Ames: Iowa State University Press. 1988. Pp. xii, 168. \$24.95.

Undergraduate science majors will probably enjoy this book and profit from its insight that scientists, lacking the objectivity said to be inherent in the scientific method, have often let their prejudices guide their observations. Historians of astronomy will note that Norris S. Hetherington's thesis is old and that he often misunderstands the evidence he presents. Time and again he himself illustrates the failed objectivity of which he accuses so many astronomers. Dipping into the past, he finds what he expects to find, even when it is not there to be found. In his preface Hetherington suggests that "the worst fate of heretics is to be ignored." I would suggest that this is an appropriate fate for this book.

Hetherington rightly calls attention to the extent to which external circumstances influence perceptions, what he calls "believing is seeing" (p. 11). He does so, however, more to punish scientists than to increase our understanding of them. To give but one example typical of many: Hetherington recounts the story of Thomas Harriot, whose drawing of the moon in 1609 depicted what he actually saw, while his drawing of the moon in 1610 seems to have been heavily influenced by Galileo's drawing published in the *Sidereus Nuncius*. For Hetherington this is an example of failed objectivity. Others, with more charity as well as more attention to historical context, would recognize that, although Harriot did rely on Galileo, he was not trying to cheat. Rather, he, like most observational and experimental scientists, appreciated how difficult it was to interpret what he thought he saw. He understood that, although his simple telescope—it was the first in England—revealed things invisible to the naked eye, it did not give a stable, clear, unambiguous view of distant reality. Unsure of his own perceptions, Harriot chose to trust Galileo. Perhaps he was wrong; perhaps he did the best he could.

DEBORAH JEAN WARNER
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LYNN SUMIDA JOY. *Gassendi the Atomist: Advocate of History in an Age of Science*. (Ideas in Context.) New York: Cambridge University Press. 1987. Pp. xiii, 311. \$34.50.

To treat ideas in their context, as this book on Pierre Gassendi attempts to do, suggests the need to focus on the social and political milieu within which this early seventeenth-century French philosopher lived and wrote. Gassendi (1592–1655) reached maturity in the 1620s, and he shared the fears of his contemporaries. In "the great darkness of these times," as Gassendi described his era to a counselor of the young Louis XIII, those fears concerned the possibility of civil war, of drift and profound instability. How that context may have related to Gassendi's profound historical

interests, his humanism, and, most significantly for the history of science and philosophy, his rejection of Aristotle is not, however, addressed in this book.

For Lynn Sumida Joy, context means the analysis of Gassendi's scientific thought in relation to his historical research, and Joy presents Gassendi's historical research as the base from which his atomism derived. Gassendi's adoption of atomism ultimately rested not on his understanding of matter but on his reconstruction of the historical Epicurus.

Until the appearance of Joy's book, there was no modern book-length study of Gassendi available in English. In filling that gap, this is a valuable, carefully argued, densely written monograph that is probably definitive in its treatment of Gassendi as a historian. His debt to humanistic historiography is carefully elaborated, and the concluding remarks on his limited influence on Isaac Newton and John Locke are judicious. Yet because cultural context, as that phrase is generally understood, is not addressed, questions remain concerning the motivation for Gassendi's researches and the purposes that his philosophy of nature may have been intended to serve.

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RACHEL LAUDAN. *From Mineralogy to Geology: The Foundations of a Science, 1650–1830*. (Science and Its Conceptual Foundations.) Chicago: University of Chicago Press. 1987. Pp. xii, 278. \$27.50.

Over the past two decades or so, a substantial body of revisionist literature has been produced dealing with the history of geology. The revisionism has taken the form, in the first instance, of a break with the received view that modern geology originated in Britain with James Hutton's concept of uniformitarianism. This view, it is now believed, represents a partisan distortion of history and originated as part of the self-justifying polemics conducted by such Victorian uniformitarianists as Charles Lyell and Archibald Geikie. Historians have come to recognize, too, that the use of such traditional historiographic models as "Neptunist-Plutonist," "uniformitarian-catastrophist," and "Genesis-geology" has led to a simplistic and impoverished understanding of the origins of geology. Instead of these dialectical models, other approaches have been used in recent years, including that of institutional history, placing the study of the earth within its context of social and cultural change. Many of these newer studies, however, are narrow or even parochial in scope and are confined to Britain.

Rachel Laudan's major accomplishment in writing this book is to have brought together a large part of the revisionist literature to create an integrated and broad diorama of the growth of geology. Her chronological scope extends over nearly two centuries, from the middle of the seventeenth century to the 1830s, and she takes in, in addition to British developments, much

of what happened on the European mainland. In doing so she not only provides a badly needed synthesis but also advances significantly the process of revisionism. The traditional view that Abraham Werner's Neptunism was one of the last stumbling blocks of modern geology is refuted and even turned upside down. Laudan argues that the essence of Werner's teachings was the concept of formations and the formulation of a program of historical geology rather than the issue of basalt lithogenesis.

Laudan's reassessment of Werner and the Freiberg school is enlightening in two ways. First, she compares and contrasts the Wernerian ideas with a preceding tradition of Continental mineralogy showing how historical geology evolved from older process-related theories of the earth. Second, she describes the enormous impact that the Freiberg school had and concludes that it represented the single most important factor in the coming of age of geology during the period from 1780 to 1830. This relocation of the cradle of modern geology from the British Isles to the European mainland is convincingly argued, and Laudan's case is strengthened by considering the revolutionary contributions to the study of fossils made by Johann Friedrich Blumenbach, George Cuvier, and other coryphaei of European paleontology.

Although Laudan pays some attention to social context, for example, when discussing the importance of eighteenth-century mining schools, her story is primarily a form of intellectual history. She uncompromisingly argues for the supremacy of the cognitive approach over the one that uses social context, emphasizing the distinction between "causal" and "historical" theories in geology. It is understandable that in a book of this scope the cognitive aspects of geology make up the bulk of the text. To omit much of the social context is justifiable, not because the advocates of sociocultural explanations have failed to demonstrate that their method has teeth, as Laudan maintains, but because case studies of the institutional and social context of geology are, at the present time, too patchy to allow for the type of unified, large-scale picture that Laudan draws. The contextual approach is indispensable, however, if one deals with, for example, differences between regional or national schools of geology. The special merit of Laudan's book is, therefore, not its contribution to methodology but its pan-European scope and its assignment of a central role to the Wernerian school in the making of modern geology.

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MARJATTA HIETALA. *Services and Urbanization at the Turn of the Century: The Diffusion of Innovations*. (Studia Historica, number 23.) Helsinki: Suomen Historiallinen Seura. 1987. Pp. 481.

Marjatta Hietala is a leading member of the younger generation of Finnish urban historians. Like many Nordic scholars, she recognizes the need to publish for a European and world readership. In this important study of the growth of municipal enterprise before 1914, she concentrates on Germany but makes comparisons with Britain as well as with Stockholm and some of the larger Finnish cities. She is careful to include an American perspective as well, particularly through the European reports of Frederic C. Howe. The resulting novel perspectives and enriched comparisons confirm the potential of the Nordic contribution to international history.

The study rests mainly on a data base derived from published statistics relating to the forty-four largest cities in the German empire between 1870 and 1920. The biggest concentration of data was achieved between 1880 and 1910, when the collection of German municipal statistics was at its peak in both quantity and quality. British efforts were less systematic, but something approaching German consistency was achieved in the last years before World War I. The statistical collections reflected a drive for emulation among the cities, supporting Hietala's view that municipal enterprise was strongly competitive, both within and between the urbanizing states of Europe. So rich are the German figures that Hietala is able to distinguish seventy-three variables. Her regression analysis allows her to group the cities in six functional categories on the basis of their occupational structure. This classification is complemented by a regional division that allows her to take account, to some degree, of income levels, although average personal incomes by city remain a frustrating unknown in this as in other urban history studies, as Hietala is the first to lament. The use of seventy-three variables, however, allows some novel associations to emerge. The metal and textile cities were consistently short of services but scored well on all of the indicators associated with dynamic growth. The eastern cities, however, tended to perform poorly across a broad front. In 1909, for instance, Strasbourg had five times as many dentists per capita as Chemnitz, the classic urban loser of the age. Meanwhile, Hietala's fascinating group of "garrison cities" exhibits amazingly high levels of military participation, for instance, 40 percent of the total population at Metz in 1895.

The later chapters concentrate on innovation diffusion, as technology and institutions spread, first from Britain and later from Germany. The early British contribution was mainly in the field of public health. The German contribution included a much broader range of institutions, and from the 1890s Germany left its mark on both Britain and America. The discussion of municipal trading is especially sensitive to national variations in concept and practice. The analysis of health, in which service provision is related to health standards, provides a completely new perspective on pre-1914 social history. Education, water, and international exchanges are all given stimulating treatments.

In short, this original book will prompt much thought in more than one urbanized country.

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ALLAN R. MILLETT AND WILLIAMSON MURRAY, EDITORS. *Military Effectiveness*. Volume 1, *The First World War*; volume 2, *The Interwar Period*; volume 3, *The Second World War*. (Mershon Center Series on Defense and Foreign Policy.) Boston: Allen and Unwin. 1988. Pp. 361; 281; 375. \$150.00 the set.

This is an ambitious project that seeks to examine the military effectiveness of Great Britain, the United States, France, Germany, Russia, Italy, and Japan during the two world wars and in the interwar period. There are twenty-one case studies, plus summary essays by Paul Kennedy on World War I, Alvin Coox on the interwar period, and Earl Ziemke on World War II. The three distinct periods naturally lend themselves to a three-volume format. Each volume can be read independently, but the introductory chapter by the editors Allan R. Millett and Williamson Murray (together with Kenneth Watman), which establishes the criteria used, is found only in the first volume, and the third volume contains two additional chapters by John Cushman and Russell Weigley that range over the entire period.

The editors asked the authors of the essays to examine military effectiveness on four different levels: political, strategic, operational, and tactical. Millett and Murray define the term "operational" as the way forces are employed to achieve strategic objectives, while the term "tactical" includes the specific methods used to fight engagements aimed at securing operational objectives. The editors admit the levels can overlap, and they do become blurred in the essays.

The editors pose specific questions—too numerous to mention—within each level of effectiveness. On the political level, these include the extent to which the military organizations can obtain an adequate share of the national budget to meet their major needs, have access to the industrial and technological resources to produce the equipment needed, and obtain manpower in the required quantity and quality. The strategic questions include the degree to which the achievement of strategic objectives results in securing the political goals of a nation and whether the risks involved are consistent with the stakes. The operational and tactical questions range from consistency of concepts and decisions with available technology to morale, unit cohesion, and relations between officers, noncommissioned officers, and men.

These are stimulating questions and in some cases hard to answer. The different authors wrestle with them with varying degrees of success, and their best answers are, not surprisingly, in their respective areas of expertise. The general impression is that some of the authors are more comfortable tackling problems at the

higher political and strategic levels and that analysis of the tactical questions is occasionally less complete. The navies do not always get their due, particularly on the tactical level, except from those authors who have written extensively on naval history. The essays do, however, provide a multitude of valuable insights and analyses, particularly on questions such as manpower and budgetary allocations that are sometimes overlooked in studies that deal mainly with operations. Much information is packed into this work that would require extensive reading in unfamiliar sources to obtain elsewhere. The authors of the chapters on Japan (Ian Nish, Alvin Coox, and Carl Boyd) and Russia (David Jones, Earl Ziemke, and John Jessup) also provide a valuable service with citations from Japanese- and Russian-language material in areas where, for many, interest in military history exceeds linguistic capability.

The tactical problems and thorny questions of leadership are tackled in a crisply written chapter by Cushman on challenge and response at the operational and tactical levels from 1914 to 1945. Cushman asked the authors of the twenty-one case studies to assign grades to their respective armies for tactical and operational performance. This is, of course, highly arbitrary, subjective, probably oversimplified, but fun. It is also something with which academics can identify. The results do not reflect an honors section. In tactical performance there were only two As (Germany, 1919–40, and Germany in World War II) and seven Bs, four Cs, four Ds, and four Fs. The Fs, predictably, are for the French, 1919–39 and 1939–40, and the Italians in both world wars. The grades for operational performance are similar: only one A (the United States in World War II), nine Bs, five Cs, four Ds, and two Fs. The French, 1919–39 and 1939–40, are at the bottom of that class, too.

The Germans, despite being on the losing side in both world wars, do very well at the operational and tactical level. They receive a B in tactical performance for World War I and a B for operational performance in each of the three chronological phases, and the authors judged that Germany deserved As for the first phases of the two world wars. Those grades are, however, only for the tactical and operational levels. The question of political and strategic effectiveness is something else. Holger Herwig, analyzing World War I, Manfred Messerschmidt, discussing the interwar period, and Jürgen Förster, examining World War II, brilliantly dissect the convoluted command relationships, the labyrinthine and often competing organizational structures, and the strategic blindness that did so much to nullify German excellence at the lower level. The results of their analyses are often scathing, and one is almost surprised that they eventually graded operational and tactical performance as high as they did. Förster, probably in reaction to those who have heaped praise on the Wehrmacht, seems almost reluctant even to admit tactical excellence, and his essay is likely to be one of the more controversial.

He argues that the militarized *Volksgemeinschaft* (defined as the nation as a whole), symbolized by an unshakable belief in the Führer, contributed to the Wehrmacht's effectiveness to a much larger extent than former generals were willing to admit after the war.

The three essays on the Germans are, in fact, so critical that the Germans seem to emerge as much more formidable in the essays devoted to their opponents. This naturally reflects the experiences of those who actually had to face them on the tactical and operational levels. Douglas Porch, for example, admits the French army in World War I could not match the Germans in the number and quality of its cadres. In a similar fashion, Millett flatly states that the U.S. Army ground forces in World War II were a "flawed instrument" (vol. 3, p. 61) that often compensated for operational flaws with logistical abundance, but on the tactical level their fighting qualities never matched those of the Germans. Nevertheless, he still gave the United States a B in tactical performance in the Second World War, but that is a composite grade including naval and air forces. In comparison, Timothy Nenninger rated the American tactical performance in World War I as a D; operational performance, C. In the interwar period, Robert Spector gave the U.S. forces a C at the tactical level and a B at the operational level.

The British had similar problems at the tactical and operational levels. In World War I, Paul Kennedy notes "a rigidity of mind and a certain lack of imagination" (vol. 1, p. 73) and, in his excellent summary chapter, points out that the critical problems occurred at the tactical level much more in World War I than in World War II. The Royal Navy appears best in Brian Bond and Williamson Murray's chapter on the British in the interwar period, and Murray notes the failure of the senior army leaders (with the exception of Field Marshals Slim and Montgomery) in World War II to grapple with the tactical problems. It is not surprising that the British receive mediocre grades: on the tactical level in World War I, a C (initially F) and Ds for the other two periods. On the operational level in the First World War they receive a D overall (actually F/D) initially, rising to C/B) and Cs in the other two periods. Kennedy, Bond, and Murray agree that the British did much better at the political and strategic level; in World War II, Murray claims they did "remarkably well" (vol. 3, p. 91), allocating their scarce resources and executing a sensible strategy.

The Italians are extremely well served in essays by John Gooch on World War I, Brian Sullivan on the interwar period, and Macgregor Knox on World War II. The three authors are clear in analyzing what went wrong and explaining the handicaps (sometimes self-inflicted) that affected Italian performance. Although the best Italian grade is a C (for operational effectiveness, 1919–39), Weigley, in his concluding chapter, asserts that the Italian army in World War I "transcended to an impressive extent the weaknesses of the Italian state" (vol. 3, p. 348).

In the line of "what went wrong," Robert Doughty is also most effective in showing how the French labored industriously at implementing what proved to be disastrous policies. David Jones, on the other hand, does much to rehabilitate the Russian performance in World War I. Jones's essay is one of the longest chapters but unfortunately says little about the navy, whose importance (or at least potential) was far from negligible in the Black Sea.

It is impossible in a short review to do justice to the subtlety and complexity of all of the essays. They are of a uniformly high standard. I would not necessarily agree with all of the arguments made, nor do the authors agree with each other. For example, while Murray's admiration of Winston Churchill is apparent, Weigley is highly critical of his bombing policy. My major criticism is one of omission. It is hard to justify including a chapter on Japan's very limited participation in World War I (although Nish's essay is excellent) and omitting essays on Ottoman Turkey and Austria-Hungary, both major participants. Kennedy does mention the heterogeneous Austro-Hungarian army as fighting to the bitter end, and Weigley also cites it as another force transcending the weaknesses of its society. The results of a critical analysis of Austria-Hungary and Turkey would have been fascinating and would obviously have added to the appreciation of the performance of their adversaries. Austria-Hungary, after all, outlasted Russia and finally signed an armistice only a week before Germany. The first volume is really incomplete without it. With that point made, one can conclude that the editors and authors have produced a worthwhile addition to the military history of the twentieth century.

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EDWARD M. SPIERS. *Chemical Warfare*. Urbana: University of Illinois Press. 1986. Pp. ix, 277. \$24.95.

L. F. HABER. *The Poisonous Cloud: Chemical Warfare in the First World War*. New York: Clarendon Press of Oxford University Press. 1986. Pp. xiv, 415. \$59.00

Even in a thermonuclear era, chemical warfare is the horror that does not speak its name. Its origins and nature remain shrouded in mythology, with totems and taboos too often substituting for analysis. L. F. Haber is an economic historian and the son of Fritz Haber, one of the godfathers of imperial Germany's gas-warfare capability. Edward M. Spiers, best known as a historian of nineteenth-century Britain's military establishment, has also been employed in the British chemical industry. Their works reflect their perspectives. Haber is less interested in the operational, moral, and human aspects of gas warfare in World War I than in presenting a case study of military innovation, concentrating on its technical and socioeconomic aspects. Spiers, on the other hand, develops an account of the fears generated

by chemical warfare into a defense analyst's advocacy of a specific contemporary policy on chemical weapons.

Haber's volume, the more focused of the two, establishes the problems of improvisation and muddle that characterized the development and supply of war gases from 1915 to 1918. Previous applications of industrial technology to warfare had been relatively systematic. They usually involved engineers and technicians rather than research scientists. They usually reflected a desire either to create a demand or to fill a specification established by armed forces. But gas was an entirely new weapon, and the only defense against it was the respirator, an entirely new device. Haber demonstrates the difficulties of developing working relationships between soldiers and scientists, who for the first time were brought into constant contact on ground neither of them dominated. Amateurism, tension, and the strategic timetables of total war contributed to a situation where decisions were too often made by default.

Despite the structural problems involved in its development and deployment, gas warfare generated a fear of the unknown. Gas killed and injured in unconventional ways, ways to which armies could not accustom themselves. Does the sensation of choking, Haber rhetorically asks, create anxieties different in essence from those involving anticipation of the suffering inflicted by shell or bullet? The Great War ended before the question could be answered.

By 1918, gas may still have been, in Haber's words, a "wartime scientific curiosity" (p. 277) with limited operational effect. Its myths, however, not only survived but continue to flourish. Most gas victims recovered and led normal lives. But that did not mean lives free of pain and illness. Neither governments nor pension systems were designed to deal with the psychological results of chemical warfare on veterans who ascribed feeling unwell to the consequences of being gassed. Gas, moreover, was increasingly perceived as a weapon able to transcend existing delivery systems and become a potentially unlimited danger to societies as well as soldiers.

Here Spiers's work takes up its argument. The strength of his book is its demonstration of an enduring fear of poison gas—a fear that has made deterrence far more important than disarmament in restricting the scope of chemical warfare. Even during World War II, Spiers insists, the most powerful checks on gas warfare were threats of retaliation and escalation. He fails to consider Rolf-Dieter Müller's position that shortages of raw materials and the success of more conventional methods of warfare contributed more to Nazi Germany's reluctance to use gas than did concern for possible Allied responses.

Spiers is on surer ground in describing the comparatively widespread use of gas in wars in which one side lacks an adequate defense or an adequate deterrent—notably in conflicts in the Third World from Abyssinia to Cambodia. Here too, however, he underplays inconvenient data such as recent evidence regarding the

"yellow rain" controversy and the apparent restraints on gas use during the Iran-Iraq war.

Spies continues with three chapters establishing the Warsaw Pact's comprehensive preparations for offensive and defensive chemical warfare, NATO's posture of relatively ignoring the unpleasant subject, and the limited prospects for effective treaties on chemical disarmament. He concludes that Europe will best be spared the horrors of chemical war by the same methods that have to date averted nuclear conflict: a credible deterrent and an extensive defense system, neither of which currently exists. The argument is more likely to reinforce preconceptions than it is to make converts. As long as chemical warfare lacks a comprehensive scholarly treatment of its history, it will remain one of the dirty little secrets of modern civilization.

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BERNARD WASSERSTEIN. *The Secret Lives of Trebitsch Lincoln*. New Haven: Yale University Press. 1988. Pp. viii, 327. \$27.50.

This is a most entertaining and suggestive book by a most imaginative and courageous author. It is, first of all, a marvelous example of what the historian as sleuth can accomplish. Bernard Wasserstein has pieced together an amazing life from an equally amazing variety of sources. But, perhaps more important, this is a book that dares to suggest, with verve, that what traditionally has been regarded as the periphery may in fact be the heart of the twentieth century. As Wasserstein is quick to admit, his is a book not about a primary character in the twentieth-century drama but about a marginal figure whose aspirations were maniacal and achievements pitiful. The work is, in terms of the conventional canon, a gargantuan footnote. By extension, however, one is led to wonder whether the footnotes in some of our more standard works of scholarship may not be more important than the texts.

The story of Trebitsch Lincoln is wilder than any fiction. It begins as the story of a failed actor and turns into that of an endless wanderer between worlds: a Hungarian Jew who became a Christian missionary to the Jews of Montreal; then a Liberal Member of Parliament in the short-lived British Parliament of 1910; a speculator in the oil business; a British civil servant in the early months of the Great War; a forger; then, with back to the wall, a self-styled spy and double agent in the latter part of that 1914-18 war. Following his release from Pentonville prison in 1919, Lincoln fled to Germany and became a protagonist in the fiasco that was the Kapp Putsch in March 1920. Forced then to leave Europe, he ended up in China, first as adviser to Chinese warlords, then as a Buddhist monk, and finally as a collaborator with occupying Japanese forces. Trebitsch was everything and in the end nothing. Wasserstein's tale, appropriately, peters out

toward its close. The sources become more scarce. In Pinteresque fashion, the silences become louder.

I must confess to partisanship in my response to this book. Those of us who have labored for any length of time in archives are well aware that historical scholarship, like the piloting of commercial aircraft, consists for the most part of long stretches of tedium. How grateful we are when we come across files that are intrinsically interesting. Many years ago, when I was scouring German archives with a particular purpose, I ordered up, at the West German Federal Archives in Koblenz, some of the papers of Colonel Max Bauer. It was there that I first encountered Trebitsch Lincoln. For days I was engrossed. I read the Bauer papers as if they were a novel. I took only one note, at the end, and this to remind myself that one day I should return to the story of Lincoln. Of course, I never did return, but I am grateful to Wasserstein for reminding me of that distant pleasure and for now putting it into a proper context.

This man, Lincoln, with his messianic delusions and endless wandering, was perhaps the quintessential twentieth-century misfit. There was something positively Olympian about the posturing and the failures. They deserved a good accounting. This has now been rendered.

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GEIR LUNDESTAD. *East, West, North, South: Major Developments in International Politics, 1945-1986*. Translated by GAIL ADAMS KVAM. Oslo: Norwegian University Press. 1986. Pp. 308.

A first glance at this textbook in international history might deter the professional historian from proceeding further. The title is awkward; the style is barely serviceable; and the opening chapters are too simplistic, particularly the effort to fit cold war scholars into dated categories. But it would be a mistake to dismiss this book because of infelicities of title, style, and occasional oversimplification. Geir Lundestad has written an important survey of international politics in the last forty years from a perspective, as John Lewis Gaddis notes in a gracious preface, different from those of Washington and Moscow.

The author identifies himself as a postrevisionist. This self-description does not do him justice. To paraphrase Thomas Jefferson, we are all—or almost all—postrevisionists today. Lundestad moves beyond this essentially American label to examine developments in the Third World as carefully as he does those in the First and Second Worlds. If the United States and the Soviet Union still receive more attention than other nations, it is simply because they were the major actors in the world scene between 1945 and 1986. Nonetheless, problems of decolonization and economic relations between North and South share attention in this

book with the nuclear arms race and superpower competition in the East and West.

Lundestad's postwar world has few heroes or villains. Although he takes into account the role of ideology in international relations, he sees the cold war more in geopolitical than in ideological terms. Joseph Stalin's restraint more than his aggressiveness is noted in the immediate aftermath of World War II, and the expansion of the United States in this period was welcomed by its European allies. Lundestad's explanations of attitudes governing the behavior of the major individual nations are a major virtue in this volume. His treatment of irritations and misunderstandings between the United States and its allies in the North Atlantic Treaty Organization is masterful.

If the author made a special effort to suppress a Norwegian approach to international politics, he was successful, perhaps too successful. The Scandinavian role in the alliance is barely visible. Lundestad does not even develop his own insightful conception of the American empire, as he has done in his earlier contributions. Although he devotes a section to the multinational corporations, he does not speculate on their effect on the status of the "invitation" Europeans made to the United States in the early postwar period.

What distinguishes this text from its many good competitors is the effective integration of the Third World into the history of the postwar years. The decolonization process here is much more than a footnote to superpower rivalry. Lundestad presents a full analysis of the independence movements as they appeared on the international, national, and local levels. He manages to provide a clear picture of similarities and differences among the new nations of Africa and Asia. His concluding chapter deals with various models of industrialization and growth, with special attention to theories concerning development and underdevelopment.

A book of wide scope and relatively few pages inevitably exposes flaws. Some events are glossed over too quickly; the Korean War, for example, had an impact on Europe that is not addressed. The Southeast Asian Treaty Organization and the Central Treaty Organization justifiably receive little space, but they deserve more explication than is found here. Citations are not necessary in a text, particularly when the volume includes a succinct but useful bibliographical essay. But, when the author introduces development economists and structural theorists into his discussions, the reader needs to have more information about these theories and theorists than Lundestad provides.

The foregoing are essentially minor caveats. This is an unusual book that merits the attention of students of international history everywhere. In particular, it should find a place on the reading lists of courses in U.S. foreign relations.

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ANCIENT

WALTER BURKERT. *Ancient Mystery Cults*. (Carl Newell Jackson Lectures.) Cambridge: Harvard University Press. 1987. Pp. ix, 181. \$20.00.

Walter Burkert's publication of his 1982 Jackson Lectures at Harvard University is another of his important contributions to our understanding of the religions of Greek, Roman, and Near Eastern cultures. Everything from his pen is welcome and an education. As a young man Burkert established himself as the premier scholar of his generation working in Greek religion; his knowledge of the sources is encyclopedic, his judgment in combining them original, illuminating, and persuasive.

One of Burkert's aims, in these four essays, is to show us that ancient "mystery" cults are not so mysterious after all. He is not certain that the traditional interpretation of the word "mystery" should be from *my(s)*, *muein* ("an initiate," "to initiate"); he points out the possible Mycenaean tradition and the formation of the word in a way that is normal for festivals (Anthesteria) and festival places (Telesterion). He focuses on the actual practices recorded for the cult and celebrations of five major mysteries: Demeter at Eleusis, the Mother of the Gods Meter (and Attis) at Pessinus and further west, and Dionysus, Isis, and Mithras in their classical manifestations. Individual chapters explore the personal needs and motives of men and women who joined a cult; the organization of the cults and how the celebrants felt "related"; what links there may have been between the cult officials and their inherited *logoi*, the justifying traditional tales or myths, and "sacred texts" (never sacred in a fundamentalist biblical or Qur'anic sense); and the effects an extraordinary, crafted, theatrical setting may have had on individual experience (the cosmos as a huge hall of mystery, the topography of the afterlife, the similarities between the experience of death and the initiate's experience of sensing himself as a stranger, "other," renewed). This survey is filled with insight, wit, and a trenchant sympathy for distressed ancient people who in their desperation might have turned to one cult or another and then, in extreme trauma (the death of a child), rejected at last all promises of hope and help offered by the initiation experience.

This study is bound to become a standard text in the history of religion and in the interpretation of individual experiences in antiquity. The book aims not so much to give detailed information, from inscriptions or scattered quotations, as it does to convey a set of states of mind and feeling, the forms of psychological motivation for participating in mysteries, the possible expectations of rewards, or, at least, relief from anxiety. It is another compelling map drawn by Burkert of those personal feelings, fantasies, and fears in an ancient mental landscape that often seems dim, remote, confused, or contradictory to us, who are reluctant to view it as precisely like our own. It creates a sympathetic anatomy of classical minds not satisfied with traditional Olympian sacrifices and pieties, state

cults, and traditional myths often queried by poets and philosophers as being beyond the private life or, where divinity was manifestly involved in private life, as being mysterious and unjust in its works ("the wicked flourish"; "I have sacrificed and prayed so often; why does this disaster come to me?"). This slender but packed volume is another powerful document in intellectual history.

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WAYNE T. PITARD. *Ancient Damascus: A Historical Study of the Syrian City-State from Earliest Times until its Fall to the Assyrians in 732 B.C.E.* Winona Lake, Ind.: Eisenbrauns. 1987. Pp. viii, 230.

Following a straightforward chronological thread, Wayne T. Pitard reviews the evidence pertaining to the city of Damascus and its immediate region until the time when Damascus became a province of the Assyrian empire. As a city-state, Damascus was a political entity as well as a settlement, and it is exclusively the political aspect of the city's history that can today be the object of scholarly investigation, because Damascus as a settlement (for pre-Hellenistic times) is either buried or obliterated, at any rate inaccessible.

The evidence available for a political history of Damascus in the early first millennium B.C. comes from two types of sources: the later manuscript tradition as embodied in the Bible and the contemporary epigraphic documents, primarily Aramaic inscriptions. This period of about three centuries (1000–732 B.C.) is the best documented and gets, accordingly, the lengthiest treatment by the author (pp. 81–189). In an interesting philological section (pp. 138–44), Pitard provides a new reading of the Bir-Hadad stela and concludes that the monument stems from a king of northern Syria and not of Damascus. The Biblical material is so preeminent that at times the discussion is more about ancient Israel than about Damascus (for example, on pages 115–25, where the author concludes that, contrary to the *communis opinio*, "there is virtually no information in the Biblical sources about Damascus during the time of the Omride Dynasty" [p. 124]).

For the second millennium B.C., the evidence is derived from contemporary cuneiform documents: the author discusses those on pages 27–80, and a good section (pp. 39–48) is devoted to the elimination of yet another portion of the evidence from the dossier, that is, the evidence dealing with the country of Apum, which, the author rightly concludes, is to be linked with Mari rather than with Damascus. The earlier periods, for which only archaeological evidence can be adduced, are covered in the introduction. It seems ironic that so much of the volume deals with the rejection of alleged evidence pertaining to Damascus, to the point that only on page 107 do we meet with "the first concrete historical information about the kingdom of Damascus."

The information is treated clearly and exhaustively, and one only misses such additional aids as a chronological chart, a regional geographical map, and photographs of the documents discussed in detail. On the face of it, the book appears to offer everything there is to know about ancient Damascus. But does it? The work affords an interesting comment on many a current "historical study" on the ancient Near East. The assurance with which these studies span vast time periods and the completeness of the pertinent information they make available are hardly matched by a corresponding increase of genuine historical understanding. That the volume by Pitard is a case in point may be shown on two counts. On the one hand, there is a curious unevenness in the coverage of the archaeological data. Thus, in the discussion of the early periods of the history of Damascus (for which no written sources are available), Pitard ranges as far afield as Mureibit on the Euphrates, whereas for the later periods the archaeological setting is basically ignored. Similarly, although for the prehistoric periods references are given to such detailed aspects of material culture as sickle blades and huts (p. 20), for the historic periods there is barely a mention of the large-scale destruction of important urban centers (p. 108).

The second point to be raised, in terms of the historical merits of this "historical study" (to refer to the title of the book), is that it fails to raise historical questions—other than the fact (significant as it may be) that Damascus simply existed. Important historical issues such as the development of urbanism, the dynamics of trade, and the relationship between the steppe hinterlands and the coastal cities are essentially ignored, except for occasional, peripheral references (for example, p. 25). While a great amount of detail is given, for example, about the relationships between Hittites and Egyptians (chap. 3), those details pertain largely to extraneous matters without a serious attempt at using the facts as a setting for a better understanding of the forces that shaped the history of Damascus. Thus, a historian will look at the book as a factual resource tool in which data are sorted chronologically in a manner that is commendably reliable and exhaustive. The "historical study" heralded in the subtitle, on the other hand, remains part of the agenda.

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BRUNO GENTILI AND GIOVANNI CERRI. *History and Biography in Ancient Thought*. (London Studies in Classical Philology, number 20.) Amsterdam: J. C. Gieben. 1988. Pp. 119.

Three chapters and three appendixes make up this study of Greek and Roman historiographical theories and practice. In chapter 1, "Theories of Historical Narrative," the differences between history designed for oral presentation and history expected to be read

are explored with regard to the search for both causes and truth. Bruno Gentili and Giovanni Cerri contrast Herodotus's kind of mimetic narration for an audience's pleasure, as defended by Duris of Samos, with the politically and philosophically useful analysis of Thucydides (as a "possession for all time") and followers of Isocrates, such as Ephoros and Theopompos, with their emphasis on writing. At a later time Polybius, who in his polemic against mimetic predecessors paralleled Platonic criticism of the poets, defended the political usefulness of his own pragmatic and objective history against both "mimetically oriented historiography" (p. 26) and the "partisan propagandistic historiography" (p. 33) of the Isocrateans.

In chapter 2, "Aspects and Trends in Archaic Roman Historiography," the authors show that, in the third century B.C., the first Romans to write Roman history used Greek, partly because the historiographical tradition was Greek but partly to counter anti-Roman views presented by Greek historians. The gap in style and content, except for the continued annalistic arrangement, between the *Annales Pontificum* and the early histories of Fabius Pictor and Cincius Alimentus must be traced to Greek influence, primarily at first that of Timaeus of Tauromenium, apparently of the "Isocratean school" and so characterized by a moralistic attitude, dramatic presentation, and love of legend and ritual. Such Greek influence continued, although by the second century anti-Hellenism resulted in a change to histories written in Latin. Gentili and Cerri then examine various theories about the way in which Sempronius Asellio assessed the two historical trends *annales relinquere* and *res pestas perscribere*. The conclusion of the authors seems flawed in their attempt to equate the histories written under Isocratean influence with annals in order to see Sempronius as the champion of Polybius's apodictic and pragmatic history.

In chapter 3, "The Idea of Biography," Gentili and Cerri analyze various ways in which ancient history and biography differ and overlap as two distinct genres. A historian such as Polybius needed to evaluate the character of a man who took political or military action, while Plutarch as a biographer needed to show the context of action in which his subject operated. Several different types of biography that were more figurative, or like portraits, both physical and moral, are examined, with due attention paid both to early examples and to the use in a poet's biography of his own works. This examination leads to an analysis of ancient (and modern) autobiographical material with regard to purpose and effect. Both eulogies and encomia are also adjudged biographical. The three appendixes expatiate on matters referred to in passing in the main body of the work: "The Pontifical Chronicle," "On Literary Genres," "Towards a Correct Interpretation of 'Phrasai' in the Fragment of Duris."

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PAGE DUBOIS. *Sowing the Body: Psychoanalysis and Ancient Representations of Women*. (Women in Culture and Society.) Chicago: University of Chicago Press. 1988. Pp. xv, 227. \$29.95.

The new approaches to the history of classical Greece pioneered, mostly in France, during the postwar decades fall under different rubrics of theory, but they share elements of technique. The structuralism of C. Lévi-Strauss, the anthropological orientation of Jean-Pierre Vernant and P. Vidal-Nacquet, the deconstruction of J. Derrida, the psychoanalytical method of Georges Devereux, as antagonistic as they may be in some aspects, nevertheless have one aim in common: to isolate the assumptions, values, and goals underlying the multilayered meanings and associations of metaphor, myth, and ritual. In their studies of Greek culture, factors of civic and moral ideology and of propaganda, the evolution of ideas, and the cultural *Gedankengut* of the age feature as prominently as battles, politics, and economics.

Page Dubois's study, admittedly indebted to the above, constitutes an attempt to formulate afresh the conceptions of the female prevalent in ancient Greece. (As Dubois rightly stresses, with minute exceptions our historical sources for the period are male-generated, so that this mirror-mirage of woman is all that can be resuscitated from the record.) Lashing out at much of feminist scholarship on ancient Greece as being—unwittingly, one supposes—in the thrall of phallus worship, Dubois comes to the following conclusions: before Plato the prevalent conceptual notions about the female centered on her reproductive function; Plato "appropriated" the metaphorical language of fertility and reproduction for his construct of the proper function of erotic love, which is concerned exclusively with homoerotic relations; profound alterations in the dominant conception of the female initiated by Plato eventually led Aristotle to his notorious formulation of the female as "the deformed male"—as Dubois sees it, the essential premise of the psychoanalytic view of women's self-conception.

Dubois examines metaphors and similes in pre-Platonic Greek texts and finds numerous ones in which the female body is likened to the earth, a fertile field, or a furrow. This finding is incontestable but hardly original or startling. The equation of the earth with motherhood and the likening of sexual intercourse to the ploughing and seeding of a field is virtually universal and a mainstay of poetic as well as obscene language. Evidently because this notion of the female is palatable to Dubois—unlike, for instance, views of women as sources of labor or as instruments of sexual release for males—she ignores the implication of coarseness in the citation she uses as emblematic of her argument, namely Creon's observation in Sophocles' *Antigone*: "There are other fields for his plough" (p. 569, Dubois 2 and 72). The fact that these words concern Creon's son Haemon, whose fiancée is about to be put to death, reveals that the statement is part of Sophocles' por-

trayal of the character as crude and heartless. The "discovery" of the earth-equals-mother simile accounts for this book's cryptic main title, which translates as "woman seen as fertile earth."

This and several other metaphorical references to woman as reproducer take up most of the book. The likening of woman's body to a clay vessel is equally undeniable, but other similes examined are less convincing. The equation of the uterus with an oven and the nurturing of the fetus with baking is marginal in the extant Greek texts. The discussion of the symbolic value of stones in myths about women is farfetched. Stones are "the ribs of the earth," the author argues. Hence, the mythical tale of Medusa, whose gaze turned men into stone, is revealing not of gynophobia but, via a secondary equation of birth and death, of respect for motherhood: "to be turned to stone by the gaze of the mother is to return to the earth" (p. 88).

The fundamental flaw, however, in Dubois's argument lies in its *petitio principii*: she selects metaphors that, in her opinion, underscore the notion of fertility and then concludes that they underscore the notion of fertility. Why, for instance, is there only passing mention of the frequent likening of "good" women to tirelessly toiling bees (Semonides 83–93; Phokylides Fr. 2; Xenophon, *Oeconomicus* 7, 32 and 38)?

The tenuousness of much of Dubois's argumentation may be seen from the reasoning in her final chapter, which focuses on Plato's alleged "desire to appropriate maternity to the male philosopher" (p. 171). One of the passages analyzed to bolster this contention is *Phaedrus* 276b. Socrates proffers a familiar argument, namely, the superiority of oral dialogue over written discourse. He uses a simile derived from agriculture, juxtaposing someone planting a "garden of Adonis" for quick results against a farmer seeding a field in proper season and patiently awaiting the harvest. The point of the comparison lies in the function of the gardens of Adonis (standing for written text). Those were planted in broken vessels and placed on the roofs by the women of Athens during the Adonia festival. The event took place in the heat of summer, at a time when plants would quickly sprout but quickly wilt. Whatever the symbolic associations of the myth and ritual of Adonis may have been, they certainly had nothing to do with fertility and motherhood. Adonis had no offspring; his mother, Myrrha, had refused marriage and seduced her own father; Adonis's lover Aphrodite, at least at Athens, was anything but a maternal figure. Yet, because any "fertile field" must of necessity stand for woman as mother, the author declares Socrates' simile to be another "gesture of the appropriation of the network of metaphors associated with women and with their role in the city. The metaphor of the fertile field . . . is here reinscribed into the relationship of pederasty" (p. 178). Even if one were to accept the author's thesis of Plato's (mis)appropriation of the metaphors of reproduction, it remains unclear how this laid the groundwork for Aristotle's well-established denial of genetic motherhood.

Dubois's study demonstrates the pitfalls inherent in the use of the techniques of modern theory without adequate intellectual discipline. In addition to the disjointed reasoning illustrated above, there are interpretations of passages out of context, unhinged syntax, and lapses into fuzzy jargon: "the female body as a space of reproduction and thesaurization" (p. 169); "habitudinization" (p. 7); "gendering creates the binarism in relation to the phallus that then accommodates other forms of hierarchical power" (p. 8).

Dubois claims to "offer another model for interpreting the Greeks" (p. 18). What that model consists of is not articulated, but presumably there is a paraphrase of it in her final recommendation: "We need to create dialogic and historical texts, to imagine human possibilities beyond the restrictive, commodified terms in which we have come to understand sexual differences" (p. 188). If that means that we should acknowledge that women bear children, and that they are not in any essential aspect inferior to men, the author will not elicit much dispute from today's historians. How these stances can enhance our grasp of Greek civilization is not demonstrated in this volume.

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NEAL WOOD. *Cicero's Social and Political Thought*. Berkeley and Los Angeles: University of California Press. 1988. Pp. xiii, 288. \$35.00.

Neal Wood has set himself a laudable task: to resuscitate the reputation of Cicero as a serious social and political theorist and to bring his works on political philosophy to the attention of a wider public. As a political scientist rather than a classicist or ancient historian, Wood has a keen sensitivity to the place of Cicero's writings in the development of republican ideology. In his initial chapter, Wood usefully reminds us of the powerful influence exercised by Cicero's treatises in early modern Europe and during the Enlightenment, when his fame was at its height, and the rapid decline of his prestige in nineteenth-century industrial society, when his "gentlemanly values" seemed obsolete and his temperate views fell under the hammer blows of Theodor Mommsen. Wood seeks to reverse Cicero's fortunes and to demonstrate the value of the Roman thinker's ideas to the late twentieth century.

As is generally acknowledged, Cicero's theoretical treatises are neither original nor especially profound. Why, then, make a case for them? Wood insists that Cicero was the first ancient thinker to provide a formal definition of the state, to distinguish it both from government and from society, and to identify its principal function as protection of private property. In Wood's formulation, "Cicero will continue to be one of the most significant figures of European culture . . . the

greatest interpreter of the political experience of the Roman Republic" (p. 55).

This study is evidently a labor of love, and a worthy one. Wood provides a lucid and intelligent exposition of Cicero's ideas on law and justice, constitutionalism, forms of government, social and moral principles, and the objectives of statecraft. The book will serve as a standard resource for teachers and students of ancient political theory.

But does this book, in fact, elevate the stature of Cicero as a political thinker or analyst? Wood conceives Cicero's purpose as a reaction to a disintegrating society in which the notables exploited imperial revenues and fought fiercely among themselves, peasants were dispossessed, latifundia manned by slave gangs dominated the countryside, and the poor were caught in a constant cycle of disruption, violence, and crime. Cicero, therefore, turned to political theory to provide solace to members of the ruling class, to remind them of an earlier, more stable past, and to recall them to the unity and harmony that once prevailed among the leadership. Wood exaggerates the dire straits of the late Republic. But, more important, he concedes that Cicero's conservative solutions were little more than wistful nostalgia, a pointless attempt to turn back the clock, and a naive encouragement for the very individualism that fragmented and atomized Rome's leadership. That hardly inspires faith in Cicero's acuity.

Mommsen berated Cicero above all for inconsistency and vacillation. Wood withholds the strictures but, in effect, confirms the analysis. His text reveals numerous incongruities in Cicero's political ideas: for example, faith in aristocratic rule and scorn for contemporary aristocrats (pp. 93–94); belief in property differential both as natural and as conventional (p. 112); the state as originating in nature but deriving from consensus (p. 128); advocacy of and opposition to the use of violence (p. 186). Those discrepancies and others remain unresolved. Wood's book may rekindle interest in Cicero's political thought but not admiration for the thinker.

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PAUL J. J. VANDERBROECK. *Popular Leadership and Collective Behavior in the Late Roman Republic (ca. 80–50 B.C.)*. (Dutch Monographs on Ancient History and Archaeology, number 3.) Amsterdam: J. C. Gieben. 1987. Pp. 281. f. 90.

In the late Roman Republic, according to Paul J. J. Vanderbroeck, the urban plebs "ruled . . . the popular assemblies" (p. 76). Ambitious politicians who were at odds with the ruling elite, and not too concerned about a reputation for behaving democratically, found in the people an alternative basis of power. All the lower offices of state were filled at elections of the popular assemblies, and those same assemblies could pass laws,

confer commands, and assign provinces. The principal reason why the urban plebs was courted by popular leaders was, however, because it offered the "only legitimate alternative" for political activity when consensus among the ruling oligarchy broke down (p. 163).

The key to Vanderbroeck's thesis is his analysis of the make-up of the voters of the late Republic. He believes that, after the passage of the agrarian laws of the late second century B.C., the influence of the rural segment of the voting population declined greatly and its participation in the political affairs of Rome became "minimal" (p. 75). As a result, the urban plebs became dominant in the popular assemblies. It was not, however, the urban plebs as a whole but only a segment of it that was significant. This segment was the *plebs contionalis*, so called because of the frequency with which it attended the *contiones*, or nonvoting gatherings that preceded assemblies where decisions were actually made. Consisting of freedmen artisans and shop owners, and working through occupational and neighborhood organizations, the *plebs contionalis* was largely free of the traditional obligations of patronage. The absence of those restraints allowed the popular leaders to develop a wholly new type of patronal relationship with this group, and the *plebs contionalis* became, in Vanderbroeck's phrase, a "public clientele."

Vanderbroeck's argument depends on a number of controversial points. Given the poverty of the sources, it is difficult to see how the *plebs contionalis* can be systematically distinguished from the rest of the voting population. Of the ninety-two instances of crowd action cited by Vanderbroeck, only thirty-eight are assemblies, and, of these, twenty-three turn out to be *contiones* (p. 144). Hence, out of the hundreds of assemblies that took place during the thirty years covered by his study, Vanderbroeck can adduce only eleven assemblies to sustain his thesis. Even some of these eleven are uncertain or irrelevant. For example, the *lex Julia agraria* (no. 34 in the author's list of crowd actions), would not have been of interest to the *plebs contionalis*, as Vanderbroeck himself admits, and the type of assembly where Sestius was attacked is unknown (no. 52). Even conceding that these eleven (or nine) assemblies might be regarded as representative of the whole period, the puzzle remains how the members of the *plebs contionalis*, who were confined to four of the thirty-five voting tribes, could have been decisively influential in the decision making of these assemblies. Moreover, it seems unlikely that the freedmen of the *plebs contionalis* were as independent as Vanderbroeck believes. They may have achieved economic independence, but can we assume they had also made the much greater leap to social, political, and psychological independence? Finally, in the absence of any extended discussion of the part played by veterans (who were mostly of rural origin), Vanderbroeck's elimination of a role for the rural plebs in the popular assemblies seems gratuitous. Surprisingly, I could find no reference to L. Perelli's *Il*

movimento popolare nell'ultimo secolo della repubblica (1982).

Despite these comments, Vanderbroeck has written an important book. He takes seriously the role of the plebs in Roman political culture and analyzes its various manifestations thoroughly. The collection and the organization of data are particularly valuable.

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ROBERT A. KASTER. *Guardians of Language: The Grammarian and Society in Late Antiquity*. (The Transformation of the Classical Heritage, number 11.) Berkeley and Los Angeles: University of California Press. 1988. Pp. xxi, 524. \$65.00.

In recent decades, historians of late antiquity have tended to focus their attention on the changes, upheavals, and innovations that punctuate this period. In the western Mediterranean the Roman empire was supplanted by Germanic kingdoms. In the eastern Mediterranean, by contrast, the Byzantine state grew out of Roman imperial institutions in the course of a more gradual, more organic evolution. At the same time, whether it was in the East or the West, Christianity transformed every aspect of private and public life. All of those changes came to a head during the third to the sixth centuries, the period under discussion in this work. The undivided Roman world of the third century, in which Christianity was merely one of many competing cults, had by the late sixth century fallen apart into a plurality of states among which Christian institutions and convictions had emerged as the principal common denominator. Of this, Kaster mentions almost nothing. Instead, the book focuses on the astonishing cultural and linguistic continuity of the late antique world and the often-criticized literary conservatism of its educated elite. Kaster describes one of the humbler groups within this elite, that of the "guardians of articulate utterance" (p. 17), that is, the grammarians who taught young people literature, literary tradition, and, hence, correct expression. A few of those grammarians, apart from teaching, also wrote school treatises and literary commentaries that proved indispensable to subsequent generations of teachers until the Renaissance and beyond. The book is divided into two parts. Part 1 describes the social and cultural role of grammarians in late antiquity with frequent reference to antecedents in the classical period. Part 2 is a meticulously detailed prosopography of grammarians during late antiquity more narrowly defined. This part will prove invaluable to anyone interested in the careers and writings of late antique grammarians. Perusal of the prosopography may bring some surprises. We do not necessarily think of men such as Augustine and John Philoponus as grammarians, but see, in particular, pages 246 and 334.

In the first part of the book, Kaster demonstrates that the professional activities of grammarians must be

understood as part and parcel of their endeavor of maintaining in society a position of modest distinction and unassuming authority. What peers and contemporaries would appreciate in a successful grammarian was a composite of professional expertise and moral worth. One could not exist without the other. Cicero's much repeated phrase about orators—that a good man (*vir bonus*) was by definition versed in eloquent speech (*dicendi peritus*) and vice versa—could thus be applied equally well to grammarians. The originality of Kaster's deeply learned and patient work of scholarship lies in his ability to discern the ancient and late antique cultural and political associations of this pair of attributes. The specific qualities of professional expertise and moral worth that late antique patrons most appreciated in grammarians were modesty and scrupulousness (p. 61ff.; p. 171ff.). Those virtues applied equally to the conduct of personal life, to the exegesis of literary texts, and to the formation of those networks of patronage that encompassed grammarians, their students, and their students' families within a finely tuned system of reciprocal favors. The merit of Kaster's argument is that it demonstrates how late antique cultural conservatism and continuity may be understood as part and parcel of the momentous political and religious changes that characterize the period. The educated elite, whether pagan or Christian, so far from resisting change or watching it passively, underpinned it with a literary and linguistic continuity that even the most radical exponents of change failed to annihilate (p. 95).

Yet the Christians did create some definitive rifts in the continuous texture of late antique literary and linguistic culture. This is one of several areas where Kaster's work invites further inquiry. He shows that Vergil's late antique commentator Servius expounded this author in the light of his perception of the nature (a certain innate quality) of the Latin language (p. 176ff.). But some Christians disagreed. Augustine thus viewed any given language as no more than a system of signs agreed on among the speakers of that language, which therefore lacked any such thing as its own "nature" (for example, p. 84ff.; cf. p. 87). The history of these conflicting opinions in early medieval Europe merits investigation. Similarly, Kaster's convincing exposition of grammarians' role in society should result in reassessments of education and literary culture both in Byzantium and in the early medieval West. This is an important book that is likely to attract readers for a long time.

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MEDIEVAL

PETER W. EDBURY AND JOHN GORDON ROWE. *William of Tyre: Historian of the Latin East*. (Cambridge Studies in Medieval Life and Thought; fourth series, number 8.) New York: Cambridge University Press. 1988. Pp. x, 187. \$39.50.

In the introduction to this examination of the work of William of Tyre, Peter W. Edbury and John Gordon Rowe remark (p. 9), "No medieval writer of history ever wrote without some axe to grind. So what were William's purposes in writing? And what was his message?" Those questions shape this first major study of the *Historia rerum in partibus transmarinis gestarum*, an unparalleled source for the history of the Latin kingdoms of the Middle East.

The study is divided into two parts. In the first part, the authors treat William's life (known only through the *Historia*), his other works (now lost), the influence of classical and Christian thought on his writing, and his sources. In the second part, they are concerned with five of the axes William ground: his attitudes toward the Latin monarchs of Jerusalem, relations between church and state, the papacy, the Byzantine empire, and the war against Islam. Using those five topics as touchstones, the authors argue convincingly that William's primary purpose was to demonstrate the transfer of authority in the fight against Islam from Byzantium to the Latin rulers of the East and to convince readers, in both East and West, of the past glories and present potential of those rulers. The authors conclude by urging scholars not to mine the *Historia* for data but to treat it as a narrative whole. That approach puts Edbury and Rowe in the philosophical mainstream of current historiography.

As an introduction to William's *Historia*, this study is admirable: cleanly presented, coherently argued, and clearly organized. The scholar who simply wants to know about that work will find this book helpful. The scholar who is curious about where the *Historia* belongs in the twelfth-century historiographic tradition, however, will be frustrated.

The authors intend to address the issue of William's place in the historiographic tradition, for they state several times that William was a product of the twelfth-century schools. The purpose of the first section of the book is in part to demonstrate that point, but the connection is not well fleshed out. There is little comparison of the *Historia* with the works of other twelfth-century historians, and there is no more than passing reference to the burgeoning modern scholarship on the twelfth-century historiographic revolution, of which Bernard Guenée is currently the grand theorist. To give one example, the authors' discussion of naturalistic explanations of causality would benefit from correlation with Nancy Partner's treatment of the same issue with regard to William of Newburgh in *Serious Entertainments* (1977). In short, a strong comparative and theoretical substructure is wanting here. A full integration of William's work into the twelfth-century tradition must await further studies. In the meantime, this book is successful in presenting William as an individual twelfth-century historian.

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DIMITRI OBOLENSKY. *Six Byzantine Portraits*. New York: Clarendon Press of Oxford University Press. 1988. Pp. xii, 228. \$55.00.

This book presents, along with a general introduction, biographies of six notable figures in the history of Byzantine influence on the Slavs: St. Clement of Ohrid (ca. 850–916), a Slavic bishop in Bulgaria; Theophylact of Ohrid (ca. 1055–ca. 1126), a Greek archbishop in what was by then the Bulgarian province of the Byzantine empire; Vladimir Monomakh (1053–1125), a prince of Kiev of mixed Slavic and Greek heritage; St. Sava (1175–1236), a Slavic archbishop of Serbia; St. Cyprian (ca. 1330–1406), a Slavic archbishop of Kiev and Moscow; and St. Maximus the Greek (ca. 1470–1556), a Greek translator who lived in Russia.

By his choice of title, Dimitri Obolensky implies that his model is Charles Diehl's *Figures byzantines* (1906), the most successful set of biographies of Byzantines written in modern times. In a fashion more common when he wrote eighty years ago than it is today, Diehl composed his study of the lives of Byzantine rulers and saints to interest both scholars and general readers. Obolensky follows Diehl's example, although he gives considerably more attention to sources and references, a modification that scholars who have laboriously searched for the source of some inadequately referenced tidbit in Diehl's book can only applaud.

By a narrow definition of a Byzantine as an inhabitant of the Byzantine empire, only one of Obolensky's six men would qualify: Theophylact of Ohrid. But Obolensky's definition is broader and better. As he demonstrated in *The Byzantine Commonwealth: Eastern Europe 500–1453* (1971), Byzantine spiritual, intellectual, and political influence extended far beyond the borders of the empire to most of Eastern Europe. Of course, just as today the majority of the inhabitants of the British Commonwealth could not reasonably be described as British, most Slavs were not very thoroughly Byzantinized. Yet Obolensky makes a good case for regarding most of his subjects as Byzantines.

Clement of Ohrid spent his youth as a monk in the empire, joined the Byzantine mission of Saints Cyril and Methodius to Moravia, and served as a representative of the Byzantine church in independent Bulgaria, much as Theophylact did when Bulgaria was ruled by the Byzantines. Cyprian of Moscow and Maximus the Greek similarly strengthened Greek (though by Maximus's time post-Byzantine) ecclesiastical authority in Russia. To call St. Sava and Vladimir Monomakh Byzantines is stretching a point, particularly for Vladimir: although Vladimir had a Byzantine mother and surname (he was probably a grandson of the emperor Constantine IX Monomachus), both he and Sava were raised as Slavic princes. But Sava, the founder of the Serbian monastery of Chilandari on Mount Athos, markedly increased Byzantine monastic influence on the Serbian church, and the interest of Vladimir's half-Byzantine ancestry is hard to deny.

The sources for the biographies also display a typi-

cally Byzantine obsession with the ideal type rather than the particular case, making them difficult material for the biographer, despite Obolensky's choice of fairly well-documented subjects. Clement and Sava are known mainly from hagiography, including Sava's life of his father, St. Stephen Nemanja; Theophylact from his own letters; Vladimir from his autobiography; and Cyprian and Maximus from more varied works, some of them their own. The personalities that emerge most clearly are those who write about themselves: an irascible Theophylact (whose dislike for Bulgarians Obolensky minimizes), a good-natured Vladimir (whose sincerity Obolensky defends), and an imprudent Maximus (on whom Obolensky mostly reserves judgment).

This is a judicious and readable book that can be recommended to a wide audience.

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CAROLE STRAW. *Gregory the Great: Perfection in Imperfection*. (Transformation of the Classical Heritage, number 14.) Berkeley and Los Angeles: University of California Press. 1988. Pp. xiv, 295. \$35.00.

This book makes an important contribution in two ways. It significantly expands our understanding of the thought of Pope Gregory the Great, and it effectively illuminates a crucial moment in the larger history of Christian spirituality. Carole Straw's study draws some of its originality from a more sophisticated methodological perspective than has traditionally been applied to Gregory's thought, a methodology that, in the words of the author, seeks to combine the skills of the literary critic, the anthropologist, and the historian. But the quality of the book rests on more than methodological ingenuity. The author displays a range of talents: a formidable grasp of the total corpus of Gregory's writings; a remarkable skill in discerning the sense of Gregory's language of spirituality; a solid grasp of the current state of Gregorian studies and of pre-Gregorian trends in Christian spirituality and theology; an acute sensitivity to Gregory's existential situation as a Roman, a monk, a prelate, and a seeking Christian; and, not least, a writing style that captures Gregory's voice yet conveys the somewhat arcane concepts of spirituality with clarity and force.

It is impossible in a short review to do more than provide a brief sketch of Straw's approach and hope that it will induce readers to discover for themselves the rich substance of the work. The study begins with a substantial introduction that establishes the secular and religious setting in which Gregory's career unfolded, reflects on his personality and education, describes the spiritual climate of his age, and identifies the essential configurations of his spirituality. Straw then devotes twelve chapters to a detailed reconstruction of Gregory's views on a set of interrelated themes that are central to his spiritual concerns and, generally, to Christian spirituality. In treating each of these themes,

Straw not only provides a full picture of Gregory's views on spiritual perfection and how it is realized but also identifies the sources of his ideas and marks the points where he modifies and enriches spiritual concepts derived from earlier sources, especially Scripture, Stoic and Neoplatonic thinkers, St. Paul, the desert fathers, Augustine, and John Cassian.

Straw's analysis of Gregory's spiritual thought leads her to conclude that his chief contribution to the history of spirituality was his "broader integration of the carnal side of life in a unified vision of reality" (p. 26). He modified a tradition rooted in Paul's teaching, which opposed flesh and spirit and demanded the obliteration of carnal life in order to achieve spiritual perfection. For Gregory the imperfect flesh was a necessary component of spiritual existence and an instrument of spiritual perfection; he found "perfection in imperfection." This formulation emerged from a view of the cosmos framed in terms of the coexistence of opposites linked by various degrees of complementarity that must be reconciled in order to achieve cosmic harmony and order. In working out the implications of the complementarity of the opposites "flesh and spirit," Gregory arrived at a spiritual vision that invested each Christian with the resources to subdue the sin-generating flesh but that necessitated the return of the "reformed" Christian to the carnal world to do good works as an expression of love.

Although Straw does not pursue the matter in any depth, the observant reader will be quick to recognize that many of the spiritual concepts she unearths in Gregory's writings prefigure important elements of later Christian spirituality, elements not implicit in pre-Gregorian spirituality. Put another way, Straw makes a persuasive case for Gregory as a seminal figure in the history of Christian spirituality. In my judgment, it is no exaggeration to say that this book will become an indispensable starting point for anyone seeking the roots and the basic contours of medieval spirituality. And it offers compelling reasons to approach the sources from fresh perspectives and to consider posing new questions about the essential character of spiritual life.

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DANIEL RUSSO. *Saint Jérôme en Italie: Etude d'iconographie et de spiritualité (XIII^e-XV^e siècle)*. (Images à l'appui, number 2.) Paris: Découverte and Ecole Française de Rome, Rome. 1987. Pp. 299. 265 fr.

Daniel Russo's heart must have sunk around the time he completed this study of images of St. Jerome to learn that one of America's most eminent interpreters of early modern culture, Eugene Rice, had just published a book on the same subject. It would be good to report that Russo's monograph has independent value despite the availability of Rice's *Saint Jerome in the Renaissance* (1985), but such is not the case. Rice's work

is so much richer (albeit not much longer) that it is hard to see who would wish to refer to Russo's study. Whereas Rice considers the fortunes of St. Jerome in the thought-world of all Western Christendom from the saint's own time until the early seventeenth century, Russo considers Jerome's fortunes solely in central and northern Italy from the thirteenth through the fifteenth centuries, and, whereas Rice proceeds by studying texts and pictures more or less equally, Russo proceeds by studying pictures almost exclusively.

Perhaps one might think that narrower would mean deeper, but Russo's monograph does not score well on that account either because its scholarship is often too thin. For example, Russo attempts to consider the evolution of the iconographic programs of fifteenth-century Jerome cycles without reference to their point of departure in the lost fourteenth-century frescoes commissioned by Johannes Andreae or the witnesses to those frescoes in Johannes's surviving verses and the *Belles Heures* of the Limbourg brothers. Similarly, he assumes that paintings of "Jerome in his Study" can be found in the northern half of Italy only between 1480 and 1500, thereby ignoring many examples beginning with a fresco done in Treviso around 1360 and ending with Venetian and Roman paintings dating from ca. 1590 and 1599. Regarding bibliography, one takes pause immediately when Russo states in his introduction that no one before him had treated the iconography of St. Jerome, when in fact detailed iconographic studies by A. Pöhlmann (1920) and R. Jungblut (1967) that aided Rice would have helped him greatly. Rice uses the Index of Christian Art, but Russo apparently does not; Rice knows the latest work on the "Penitent St. Jerome" dubiously attributed to Fra Angelico, but Russo apparently does not; Russo "would like to know" how Jerome appears in Bolognese manuscript illumination, but Rice actually shows us a specimen; and so it goes. Granted that Russo's presentation has its moments of interest, all of his main themes—the emergence of Jerome as learned doctor and cardinal, "Jerome with his Lion" and "Jerome as Penitent" as manifestations of Franciscan-style spirituality, "Jerome in his Study" as an icon of Christian humanism—are treated by Rice, who in every case displays greater learning, broader scope, and more penetrating insight. Certainly this unfortunate beginner was the victim of bad luck in having to be compared with a scholar who dwarfs him, but just the same Russo should have stayed longer in his study.

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UTA-RENAME BLUMENTHAL. *The Investiture Controversy: Church and Monarchy from the Ninth to the Twelfth Century*. (Middle Ages Series.) Philadelphia: University of Pennsylvania Press. 1988. Pp. xiii, 191. \$37.95.

This book is Uta-Renate Blumenthal's own translation of her *Der Investiturstreit*, first published in Germany in

1982. The only substantial change is a major revision and extension of the last chapter on the controversy over investitures in England, France, and Germany under Gregory VII's papal successors. The select bibliographies, which are an invaluable feature, have been updated; four good maps have been added. Blumenthal offers the student a manageable course book and the mature scholar a comprehensive survey of the results of recent investigations.

The subtitle indicates the book's subject more precisely than the main title. Well over half of the work is devoted to developments before the 1070s when, at a date that is currently under discussion, royal investiture of bishops and abbots with the ring and the staff began to be an express issue between popes and kings. On this issue, Blumenthal deals sensibly with the debate raised by Rudolf Schieffer in *Die Entstehung des päpstlichen Investiturstreits für den deutschen König* (1981). Blumenthal rightly, in my opinion, concurs with Schieffer's conclusion that, in 1075, Gregory VII issued no general prohibition of investiture, but she also cites Friedrich Kempf's caution that the matter is far from closed. Even Pope Urban II was not rigorous about lay investiture. Only after the end, for all practical purposes, of the Guibertine schism in 1100 did lay investiture persist as the outstanding issue between the papacy and lay rulers. One could argue that only then was there strictly an "investiture controversy," because lay investiture became the topic for general debate while all parties were taking stock of more fundamental and long-term changes in ideas, structures, and societies, whether royal, stemming from Ottonian and early Salian kingship, or ecclesiastical, stemming from the monasteries, the church at large, and ultimately the papacy. Blumenthal rightly devotes much time and attention to these deeper problems. One's only regret is that, in proportion, even in her translation she leaves so little room for the early twelfth century on which she has elsewhere shed such light. One asks for more, for example, about the short- and long-term effects of the concordat of Worms (1122), especially on the German church and society. If, as Blumenthal duly notes, the Emperor Henry V's original charter is still preserved in the Vatican archives, it is no less significant that, by the end of the twelfth century, the corresponding records had wholly disappeared from Germany.

Perhaps the best parts of the book are those devoted to the stages by which the papacy and its means of government developed between Leo IX's consecration in 1049 and Urban II's great councils of Piacenza and Clermont in 1095. Gregory's predecessors, including the so often underestimated Alexander II, are tellingly and discriminatingly presented. The remarkable lack of institutional developments under Gregory VII clearly emerges, as does the stature of Urban II. Blumenthal not only clarifies the course of the "investiture

controversy" as such but also sets it satisfyingly in its wider and longer context.

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SHARON K. ELKINS. *Holy Women of Twelfth-Century England*. (Studies of Religion.) Chapel Hill: University of North Carolina Press. 1988. Pp. xx, 244. \$29.95.

In this book Sharon K. Elkins examines the relationships of religious men and religious women in the formative years of post-Conquest female monasticism. Elkins describes a twelfth century full of experimentation with religious forms. She argues, against much conventional scholarship, that religious men supported the efforts of religious women to lead a variety of religious lives, that hermits, canons, and monks cooperated with women to help them achieve their goals, and that the women welcomed the continuing participation of these men in their lives.

The book is chronologically organized. Part 1 (1066–1130) outlines the post-Conquest monastic and eremitic outlooks for women; part 2 (1130–1165) deals with the growth of female monasticism and examines the patterns of male-female associations within newly founded establishments; part 3 (1165–1215) discusses the growth of ecclesiastical regulations and their implications for the new institutions.

The most valuable portion of the book is the central section. Here Elkins deals separately with the development of new monasteries in the north and in the south between 1130 and 1165. She shows how varied were the ways by which monasteries for women were begun in the south (there were thirty-nine foundations in the south during this period) and how informal much of the development was at first (only later was it regularized with charters and agreements). Whatever the genesis of the monastery, however, after its establishment small numbers of religious men remained part of the convent, performing significant functions for the nuns. The men lived alongside the women, and their guidance and aid were welcomed.

In the north this same thirty-five-year period saw the growth of forty-six new monasteries in a region where it is probable none had existed before. Twenty-eight of those monasteries adopted forms that included lay brothers, and many also supported canons and priors. The benefactors of northern female monasticism were attracted to Cistercian and Gilbertine models, and those models likely defined the nature of male-female relationships in most of the newly established houses. Lay brothers did much of the manual work, and a prior or master ran the house along with a prioress. Other northern nunneries (Benedictine, Premonstratensian) may also have incorporated men into their organizations in the twelfth century, when the rigors and isolation of northern life made such a choice reasonable. So in the north as well as the south, if for more

specific reasons, the cooperation of men and women in the religious life was a reality.

The book concludes on a note of withdrawal. The growth of new foundations dried up, regulations allowed no more new houses with two sexes, existing practices were tightened to eliminate interaction between the two sexes, and recluses (now likely to be enclosed anchoresses) were also placed under much stricter control.

A discussion of eremitic women at the beginning and end of the book is designed to serve three purposes. First, it makes the book's title more legitimate (though the study still avoids other kinds of "holy women," such as consecrated virgins who may have lived at home). Second, Elkins tries to show that the ground for the mid-century expansion of female monasticism was laid by hermitesses in the period from 1066 to 1130. Finally, she argues that English hermitesses and anchoresses shared experiences similar to those of their conventual sisters with regard to the gradual loss of the variety of forms that were tolerated earlier in the century.

Elkins makes too much of the theme of an early twelfth-century eremitism that prepared the ground for female monasticism. Even where such eremitism existed, the available evidence does not support the weight of her analysis. On the other hand, she misses an opportunity to strengthen her argument on male-female cooperation when she fails to point out that anchoresses always were dependent on men, by nature of their enclosure, and that those men were and remained supportive even after the period of greater regulation began. There was almost never antagonism or misogyny here but rather care and concern.

All that is not to detract from the many fine aspects of a scholarly and competent book.

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RUDIGER FUCHS. *Das Domesday Book und sein Umfeld: Zur ethnischen und sozialen Aussagekraft einer Landesbeschreibung im England des 11. Jahrhunderts*. (Historische Forschungen, number 13.) Stuttgart: Franz Steiner. 1987. Pp. 422. DM 128.

In this sprawling study, Rudiger Fuchs attempts to use the evidence of *Domesday Book* to analyze the ethnic composition of England after the Norman conquest. More than half of his book, however, is prolegomena to his main subject. Ethnicity for Fuchs has both biological and ideological elements. After a rather abstract introductory chapter on ethnicity and historiography, Fuchs's search for the origins of "the ethnogenetic process" that created Anglo-Norman England begins, therefore, with the arrival of the first Germanic-speaking peoples in late Roman Britain. Two narrative chapters follow, which emphasize ethnic conflict as a key to such turning points as the failures of Aethelred II and the succession crisis of 1051–52 but which basically

tread very well worn paths. Nor does his fourth chapter, on the aims and intentions of the Domesday survey, break much new ground. Here as elsewhere Fuchs follows the best modern authorities closely, but sometimes without fully grasping the challenge those authors are mounting to the very traditional picture that Fuchs himself is presenting.

The originality of the book lies in chapter 5, when Fuchs turns at last to examining the ethnic composition of Domesday England. The trouble here, however, as Fuchs himself points out at length, is that *Domesday Book* in fact tells us relatively little about ethnicity. Systematic classification of persons by ethnic group was irrelevant to the concerns of the survey, and, even where labels such as *francus* or *anglicus* were employed, it is not always clear what was meant. The difficulties of inferring ethnicity from personal names are extreme, and, in any event, the names of the vast majority of England's inhabitants in 1086 were never recorded in the survey in the first place. Fuchs's chapter therefore becomes a discussion and analysis of those Domesday entries that do appear to make some reference to ethnicity. This discussion is carefully and cautiously done but by its nature can produce only modest results. In his conclusion, however, Fuchs returns to the grander aspirations of his introduction and reflects briefly on the success with which English kings, pre- and post-1066, managed to create a common sense of "Englishness" within such an ethnically heterogeneous population. This development the author sees as marking an important stage in a Weberian process of nation building.

One can only admire an author as honest and painstaking as Fuchs, and there is certainly some useful material in this book for Domesday specialists. Taking inspiration from Peter Sawyer's arguments, Fuchs finds some interesting examples of how the more compressed entries of *Domesday Book* obscure cases of Anglo-Saxon continuity in landholding, which are revealed in the *Liber Exoniensis*. There is a lengthy discussion of sokemen and a number of helpful lists and maps of identifiable Flemings in 1086, of persons surnamed *danus* or *dacus* (the Dane) in 1066 (a surname that does not appear in any entries of 1086), and of the Bretons resident in 1086 in Cornwall. For German-speaking readers, this book will function as an up-to-date guide to the Domesday literature to the end of 1985, although its style is not likely to recommend itself to English-speaking readers whose German is hesitant. Sadly, Fuchs's book appeared too soon to take full account of the collection *Domesday Studies* edited by J. C. Holt. Holt's own essay, in particular, seems likely to prompt a reassessment of the basic purposes of the Domesday survey and might therefore have altered the shape of Fuchs's own investigation in salutary ways. It is a tribute to the very high quality of contemporary Domesday scholarship, however, that a book published in 1987 could already be in some respects outdated.

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IRMGARD FEES. *Reichtum und Macht im mittelalterlichen Venedig: Die Familie Ziani*. (Bibliothek des Deutschen Historischen Instituts in Rom, number 68.) Tübingen: Max Niemeyer. 1988. Pp. xvi, 525. \$84.00.

Irmgard Fees analyzes the rise of the Ziani family of Venice, well known through its great wealth and through the dogeships of Sebastiano Ziani (1172–78) and his son Pietro (1205–29), to determine whether it was typical for new families to arrive at leadership on the basis of landed wealth and ecclesiastical connections judiciously employed in commercial enterprise (see G. Cracco, *Società e stato nel medioevo veneziano (secoli XII–XIV)* [1967]) or on the basis of successful commerce and banking turned into political power and landed wealth (see G. Luzzatto, *Studi di storia economica veneziana* [1954]). She also addresses the problems of the volume and commercial or feudal origin of early capitalism posed by Werner Sombart and of the impact of the new families on Venetian institutions. She contributes to the resolution of those problems through an exhaustive study of the sources in the critical period of the family's ascendancy from the second half of the eleventh to the middle of the thirteenth century. She presents her analysis in five chapters on the family's history, its commercial and financial activity, its real property, its relation with the church, and its political and social position. Those chapters are supplemented with six tables, twelve maps, and two illustrations. The summaries of the known documents relating to the Ziani family between 1079 and 1351, a total of 406 documents, are given as appendix 1. Two further appendixes present critical notes on the family's city real estate and the chronology of the counts of Arbe (Island of Rab) from Ranieri Polani, 1143, to Marco Ziani, 1254, respectively. The index of places and persons covers the summaries as well as the text.

The investigation of Fees proves that the Ziani family acquired valuable city real estate and large landholdings in the territory of Venice with the profits from large-scale commercial operations and that this commercial success, too, was the basis for their rise to political prominence (pp. 228–30, 236, 237). Their relation with the church was that of benefactors more than that of beneficiaries. They exemplify the rise of the middle class into positions of power, not in conflict with but as an addition to the old aristocracy, represented by Doge Vitale II Michiel, murdered in 1172. At the time of the murder, Sebastiano Ziani was a *iudex*, or judge, that is, a member of the leadership group and a participant in the change that made the dogeship elective by the Ducal Council. In addition to Sebastiano, Fees pays particular attention to Pietro and his son Marco. She sheds light, marginally, on the relations of Venice with Byzantium, the Hohenstaufen, and the Normans. Significant, too, is her reconstruction of the Ziani family's ownership of sea-salt pans, shares of *fundamenta*. Incidental fruits of her research are the recognition of the significance of hospitals in Venice in the early thirteenth century (p. 224); the origin of the

famous Palladian San Francesco della Vigna (pp. 218–20); and the marriage of Pietro Ziani to Constance, the daughter of the last Norman king of Sicily, Tancred, in or after 1221, when the Hohenstaufen Frederic II was generally recognized as king, rather than in 1213, when the marriage might have had anti-Hohenstaufen significance (pp. 25–27, 253). This book is the first product of a very fine scholarship aided by the counsel of Helmut Beumann (Marburg) and the German Study Center in Venice (Beck Kessler, von Stromer).

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MODERN EUROPE

MICHAEL MULLETT. *Popular Culture and Popular Protest in Late Medieval and Early Modern Europe*. New York: Croom Helm. 1987. Pp. 176. \$49.95.

The author dedicates this book to his students at the University of Lancaster, and it reads like material for a lecture course, based on secondary works rather than on research in primary sources. Michael Mullett's chief aim is to knock down the supposition that late medieval and early modern Christianity was a form of social control. He observes, quite rightly, that to treat every demand for strict morals as social control is to assume that popular culture is nothing more than bucolic disorder. In his view, rural religion in the late Middle Ages was popular in the sense that it was an accepted way of "getting on with one's neighbors" (p. 41) and played an important role in creating unity and limiting violence among villagers. The religious elements of popular culture, urban as well as rural, included notions of economic justice that derived from centuries of Christian preaching and that were still alive among the people in the eighteenth century, when the elite was turning to the ideas of Adam Smith. "Justice" in this sense, defining a line not to be crossed by lords and rulers, was the basic component of an ideology of popular protest that is to be found again and again in the great peasant revolts of the era, an ideology that Mullett accordingly sees as reformist rather than revolutionary. If one can speak of social control in connection with the great popular preachers, such as Girolamo Savonarola, it is as the reverse of what is usually meant, since Fra Girolamo's "bonfires of vanities" may be seen as part of a "popular puritanical campaign" (p. 144) to prune back the extravagances of an elite given to flirting with neo-pagan ideas. In that line of argument, the changes wrought by the Reformation and the Counter Reformation are to be understood as an interplay between popular and elite cultures, not as the imposition of one on the other.

Those observations would carry more weight if the author were more careful to get his facts straight. For example, weavers were indeed "to the fore" (p. 17) of revolts in Ghent but not in Florence. The Wittelsbachs did control a bishopric, but it was Cologne, not Liège. That the Sybils prophesied the coming of Christ is

indeed nonsense, but Marsilio Ficino, humanist and spokesman for an elite culture though he was, did not originate the idea. And many of the German "plebeians" who in the 1520s opted for the "most conservative of the Continental Reformations" (p. 161) were, in fact, Zwinglian, that is, not so conservative. Mullett cites Donald Weinstein's *Savonarola and Florence* (1970) but speaks of Savonarola as preaching about the coming of a "new Cyrus" between 1492 and 1494, heedless of Weinstein's argument that such prophecies postdated the coming of Charles VIII in 1494.

More important, Mullett does not engage his opponents directly, either by name or by a summation of their arguments. If one wishes to show, in the face of much of the work that has been done on visitation records, that peasants may be presumed to have known the rudiments of the Christian faith, it will not do merely to offer the suggestion that St. Vincent de Paul "had to start virtually from scratch" (p. 43) in parts of France because his preaching followed a long period of civil war. There is in fact a case to be made that one's estimate of how sharp the contrast was between reforming clerics (Protestant and Catholic) and their flocks depends a good deal on one's estimate of the religious content of popular culture and that some influential studies (for example, Robert Muchembled's *Popular Culture and Elite Culture in France* [1985]) tend to understand popular culture as the residue that is left after Christianity is subtracted. But to meet arguments of that kind on their own level requires a lot more than an impressionistic essay, even a better one than this one is.

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MARTIN L. PINE. *Pietro Pomponazzi: Radical Philosopher of the Renaissance*. (Pubblicazioni del centro per la storia della tradizione aristotelica nel Veneto, Saggi e Testi, number 21.) Padua: Antenore. 1986. Pp. 381.

In an introductory essay to this volume, Martin L. Pine succinctly reviews modern scholarship on Pietro Pomponazzi. He makes much of Antonino Poppi's insistence that Aristotelians like Pomponazzi were not mere commentators but had philosophical positions; their professions of faith thus hid their disbelief (pp. 31, 359). Pine's own thesis is that Pomponazzi's disbelief is unusual among Renaissance Aristotelians. In setting forth this "radicalism" (p. 34), Pine stresses both Pomponazzi's attacks on Christian teaching and the naturalism of his presentation of Aristotle and the Aristotelian tradition. Only philosophy can reach "absolute truth"; the "truth" of religion is the practical function of controlling the masses achieved by propagating fables about rewards and punishments in an afterlife (pp. 34–35, 52–53, 115–23, 357–59).

Pine examines Pomponazzi's thought thoroughly on three key topics, namely, immortality, miracles, and

free will, arguing that Pomponazzi thought that philosophy discredited all three doctrines (pp. 33–35). The major chapters of the book cover those three topics. Chapter 1 studies the *De immortalitate animae* (1516). By setting forth Pomponazzi's scheme of metaphysical hierarchy, Pine well illuminates the book's argument. Unlike some, he admits Pomponazzi's philosophical shortcomings. For example, he underscores in admirably honest fashion the lack of clarity in Pomponazzi's conception of the agent intellect as a single substance, suggesting that Pomponazzi may thereby have compromised the human being's capacity to reach knowledge (pp. 75–78). He insists that Pomponazzi believed that he had demonstrated the soul's mortality (pp. 103, 109). Accordingly, Pine dismisses as "an unconvincing hedge" Pomponazzi's statement that the question of the soul's immortality or mortality is a *problema neutrum* (pp. 111–12). In chapter 2, Pine treats the subsequent attacks on Pomponazzi's *De immortalitate* and his replies in the *Apologia* (1518) and the *Defensorium* (1519), and he points out epistemological problems in the *Apologia*.

In chapter 3, Pine examines Pomponazzi's naturalistic explanation of miracles as found in the *De incantationibus* (1556) and underscores that Pomponazzi even gave like explanations for the rise and imminent fall of Christianity. Finally, in chapter 4, Pine analyzes the *De fato* (1557) to show that, although Pomponazzi accepted in Books 1 and 2 a fully deterministic universe, he adopted a new method in Books 3 and 4 (p. 301). He then accepted but reformulated Christian beliefs, including God as an omniscient and omnipotent creator. To save human freedom within the structure of Christian theology, Pomponazzi proposed that God's omniscience is deliberately self-limiting, prescinding from the determinate aspects of the future. God thereby knows the future only indeterminately and leaves everything contingent (pp. 308–17). Pine's conclusion is that on all three topics—the soul and its knowledge, miracles, and human freedom—Pomponazzi's thought reveals "paradoxes" (p. 344) and "antinomies" (p. 346) but "an essential unity" (p. 359).

This is a provocative and well-written book. There is much here with which to argue, but Pine has stated his challenge well. Those who view Pomponazzi as an orthodox, believing Christian will have severe problems with some of the texts that Pine has skillfully used to argue his case. There are others that can be interpreted, I believe, in more than one fashion. In any case, Pine has greatly enriched scholarship on Renaissance philosophy by this ambitious and very important monograph. Renaissance scholars in all fields will gain much from studying it carefully.

One criticism seems in order. It is misleading to suggest that Averroës's views on religious truth were brought to prominence by John of Jandun, since those views are expressed primarily in the *Decisive Treatise*, a work that Jandun could not have known. I do not think that Averroës's position on the relationship of faith and reason and that of the "Averroist" tradition are identi-

cal (pp. 24–25). That tradition formulated its own theory.

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ANSLEY J. COALE AND SUSAN COTTS WATKINS, EDITORS.
The Decline of Fertility in Europe: The Revised Proceedings of a Conference on the Princeton European Fertility Project. Princeton: Princeton University Press. 1986. Pp. xxii, 484. Cloth \$55.00, paper \$14.50.

In the past twenty-five years, the Princeton project on the decline of fertility in Europe has rewritten the history of the demographic transition through a massive effort at data collection and interpretation. Ansley J. Coale helped a remarkable combination of American and European scholars to produce seven monographs on fertility decline in particular nations, plus numerous descriptive, technical, and interpretive articles. In 1979 a conference at Princeton University summarized that research and analyzed its results. This "summary volume," a collection of papers from that conference that have been revised in the light of its discussions, is itself a substantial improvement in our understanding of recent demographic history.

Massimo Livi-Bacci sums up the major achievements of the project: "We now have a precise geography of the onset of fertility decline throughout the provinces of Europe; we know how and where a substantial fall from high and relatively stable levels of fertility has taken place, mostly in the late nineteenth and early twentieth centuries; we know also that after the initial fall a continuous and irreversible decline has taken place until very low levels of family size have been reached" (p. 182). Other major findings reported and more fully described in this volume are the dating of the beginning of the decline in most of Europe between 1890 and 1920; the demonstration that rural marital fertility was nearly universally lower than urban marital fertility, began its decline later and more slowly, but eventually narrowed the gap so that urban-rural differences are now smaller than before the decline; and the new recognition of cultural and regional differences as major causes of variance in demographic behavior across Europe. Perhaps the most significant finding is that the original theory of the demographic transition, a variant of post-World War II modernization theory, cannot explain differences either in the timing of the decline or in levels of fertility. Allan Sharlin summed up that failure by abandoning altogether the search for a general explanation of the timing of fertility decline as "an illusory quest" (p. 258).

The value of the Princeton project as the definitive description of fertility decline lies in the mass of data the collaborators have unearthed, refined, and published. These data are gathered in the monographs dealing with specific nations as a permanent demographic reference. This book can be considered both a summary interpretation of those data and a primer for

their use. Tables covering seventy-three pages show the progressive decline in fertility, as well as a gradual rise in levels of nuptiality, for over six hundred provinces in Europe between about 1870 and 1960. The data are expressed in indices developed in the project, which manipulate figures on births and population distribution into measures of fertility that may become standards for future research. Both mathematical and common-sense explanations of the indices are given by Coale in the first two chapters. Remarkable graphic presentations throughout the volume compress literally thousands of calculations into forms intelligible to nonspecialists: scatterplots of changes in urban-rural differences over time (pp. 238–48); comparisons of fertility control among a variety of European aristocracies before 1900 (pp. 187–88); clusters of ellipses demonstrating the regional diversity and changing distribution of fertility and nuptiality across Europe from 1900 to 1960 (pp. 61–63); and a small graph that summarizes the entire demographic transition (p. 27). Thirteen colored maps of Europe show the distribution of the indices in each of the provinces. Other graphs and tables neatly summarize regional variability and decline in infant mortality rates (pp. 212–13) and changes on the national level in proportions of women married over the past century (pp. 316–17).

Because the data have been treated and are presented in such a precise and exhaustive fashion, the authors are unsatisfied with the hazy generalizations of demographic transition theory. Susan Cotts Watkins notes the ideological origins of standard transition theory in the postwar West (chapter 11). John Knodel and Etienne van de Walle reverse the typical policy derivations made by modernizationists who advocated economic development rather than fertility planning as the panacea for Third World population growth (chapter 10).

Like all good summaries, this volume also points the way for the next round of investigation into recent demographic history. The contributors have much that is valuable to say about the nature of the “natural” fertility regime prevailing in Europe before family limitation became widespread; most notable is Livi-Bacci on the upper-class forerunners of modern fertility control (chapter 3). But because the Princeton project depended on census data, the rather recent universalization of census taking in Europe has impeded systematic investigation into the nature of fertility behavior before about one hundred years ago, when the decline was already beginning to manifest itself. The thoroughness of this project’s exploitation of aggregated statistics has probably brought that method nearly to the end of its usefulness for Europe. Knodel, who has been part of the project from its inception and who wrote the monograph on German fertility decline, now points the way forward. In chapter 9, he summarizes some findings from the demographic exploitation of German village genealogies, reworked into family reconstitutions; his full monograph, *Demographic Behavior in the Past: A Study of Fourteen German Village*

Populations in the Eighteenth and Nineteenth Centuries, has just been published (1988) by Cambridge University Press. The individual-level data covering the past two centuries allow Knodel to break up the Princeton project’s indices further into their component parts. He shows that apparently stable marital fertility throughout the nineteenth century masked rising fertility among young married women and falling fertility among older married women in a variety of rural village settings. He argues that “natural fertility” probably rose with general improvements in health, while the beginnings of family limitation were evident toward the end of the childbearing years. He supports those conclusions with sophisticated refinements on the original Princeton measures. Those data shed some light on the thorny problem of the relation of declining fertility to declining mortality, one of the primary causal connections claimed by demographic transition theory and rejected in this volume. German village women whose previous children survived were likely to stop bearing children earlier than those with child deaths in the family.

Reconstitutions offer the best possibilities both for elucidating the variety inherent in demographic behavior before the decline in fertility and for understanding the specific factors that conditioned the decline. Until the results of many such studies are available, the Princeton project’s publications will remain the standard interpretation.

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RENE GIRAULT AND ROBERT FRANK. *Histoire des relations internationales contemporaines*. Volume 2, *Turbulente Europe et nouveaux mondes (1914–1941)*. (Collection relations internationales contemporaines.) Paris: Masson. 1988. Pp. 279. 140 fr.

This study of European international relations is faithful to the method of the “school” of Pierre Renouvin and Jean-Baptiste Duroselle. The emphasis is on structures, economic and financial underpinnings, ideologies, and climates of opinion—the *forces profondes* of the era. With economy and that admirable skill in synthesizing that marks French historical studies in general, René Girault and Robert Frank discuss in an informed, clear, and refreshingly French way the complex story of Europe’s decline from the summer of 1914 to the end of 1941. This French focus is hardly surprising, given the student audience they evidently have in mind. But the book could provide a welcome illumination to those brought up on English-language manuals. So much of the general history of that time is written from a British or a vaguely North American point of view that it is useful to have a work recalling the fact that not so very long ago France was still at the center of great power politics (although the authors do not hesitate to note French dependence on British actions by 1939). This perspective, in an era inclined to con-

ceive the two wars as being, above all (at least so far as the West is concerned), manifestations of the Anglo-German struggle, is no bad thing. France is properly restored to the place it actually had, one that "Anglo-Saxon" surveys tend sometimes to overlook.

And then, without slighting monographs in other languages, these writers draw attention to French scholarship in the field of international relations. Indeed, both the text and the literature cited reflect a cosmopolitan approach. No doubt a *chasse à la petite bête* could turn up a few specimens, insignificant and not so insignificant—for instance, English-speaking readers may find irritating the persistent use of the word "Yankee," even in a Latin American context; others may think the reference, say, to the profound change of opinion in Great Britain after Munich too exiguous. And is it true that the crises of fifty years ago are "well known" (p. 230) in 1988? Is it true that as late as 1929 or so the "Anglo-Saxons" were bent on avoiding or keeping clear of "the French militarist hegemony" (p. 184)? Is it sufficient explanation to say that the March 1939 flap in London about an imminent German threat to Romanian security was a Paris "ruse" (pp. 236–37)?

Obviously much has to be telescoped, or merely alluded to, and much else excluded. But, given the constraints of a text that is fewer than three hundred pages long, this book is very possibly the best short account of the subject to date. One may hope that, as an acute, richly informed analysis of the economic, financial, cultural, and political factors making for the fatal long decline during the final years of independent Europe's international relations, Girault and Frank's book will soon appear in English translation.

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VICKI CARON. *Between France and Germany: The Jews of Alsace-Lorraine, 1871–1918*. Stanford: Stanford University Press. 1988. Pp. xiv, 278. \$35.00.

Alsace-Lorraine, during the almost fifty years it was ruled by Germany following the Franco-Prussian war, presents an interesting example of conflicting national loyalties. The identity conflict contained unique elements for the Jews of the region, on account of shifting proclivities toward anti-Semitism in France and Germany and their own uncertainty about where their best interests lay. Although at points of crisis, from the early years of the nineteenth century through World War I, Jews elsewhere in France and Germany clearly sided with the nation of which they felt themselves a part, those of Alsace-Lorraine were cast in an in-between position. Their situation raises a number of questions for the historian. Given a local non-Jewish population that was itself split, which side did the Alsatian Jews favor and why? Did the various segments of the Jewish population agree? And did their views change along with changing circumstances? The earlier literature has

tended to generalization with regard to Jewish attitudes. It is the merit of Vicki Caron's fine monograph that it presents far more complex and nuanced answers.

The Jews of Alsace-Lorraine before 1871 were considerably less assimilated than were their coreligionists elsewhere in France. Especially in rural areas, they continued to speak Alsatian Yiddish, remained religiously observant, and maintained few social contacts with non-Jews. An urban elite had gradually developed, however, and had become attached to French culture. Members of this group, who possessed a well-developed French national identity, predominantly chose the option of resettlement in other regions that were to remain under French rule during 1871–72. Those who stayed became exponents of *revanche*, continued to speak French, and provided their children with a French education. They persisted in the belief that the Jews' best interests lay with France, which had first given them full political equality at the time of the French revolution, and not with Germany, where the record on the subject was more checkered and where informal discrimination remained widespread. In succeeding years, additional Alsatian Jews emigrated to France, though increasingly larger numbers, especially those in rural areas and those seeking to evade German military service, chose emigration to the United States instead.

As native Jews moved out of what was now called the Reichsland, German Jews, more favorably inclined to all things German, immigrated into it. German Jews advocated a policy of accommodation, which at first encountered resistance from the remaining native Jews but made more sense once France became suspect during the years of the Dreyfus Affair and once the German administration proved that its treatment of the Jews was not greatly worse than its governance of Reichsland inhabitants generally. Locally, an alliance between pro-French Catholics and pro-French Jews failed on account of the clash between their respectively conservative and liberal political views. Yet the latent persistence of Francophile sentiments became apparent once more during World War I, which placed Alsatian Jews in German trenches against their close relatives on the other side. Most of them welcomed the victory of France, which had by now put the Dreyfus Affair behind it. Ironically, it was the German Jews who had settled in the region after 1871 who now opted to remigrate eastward into postwar Germany.

Caron makes good use of local archival sources and supports her conclusions by statistical evidence as well as by contemporary newspaper accounts. The introductory chapter neglects Jay R. Berkovitz's dissertation, "French Jewry and the Ideology of *Régénération* to 1848" (1982), and the concluding chapter makes the questionable assertion that a common (but differently directed) patriotism served as a unifying link among Western Jews. The similarity of political attitude, I believe, served rather to engender disunity, as Caron's book itself illustrates. These minor faults, however,

detract little from a competent, well-written, and valuable study.

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FRANZ KNIPPING. *Deutschland, Frankreich und das Ende der Locarno-Ära 1928–1931: Studien zur internationalen Politik in der Anfangsphase der Weltwirtschaftskrise*. Munich: R. Oldenbourg. 1987. Pp. vi, 261.

The French diplomat Jacques Massigli presciently discerned the crucial distinctions among the much-celebrated Locarno "spirit," "Geist," and "esprit"; indeed, the agreements of 1925, which were supposed to usher in an era of détente, instead accelerated Britain's detachment from continental affairs, Germany's efforts for treaty revision, and France's fitful retreats. In his monograph on German-French relations during the "années tourmentées" between 1928 and 1931, Franz Knipping investigates the aftermath of Gustav Stresemann and Aristide Briand's efforts to create a durable peace between Europe's long-bitter enemies. Although acknowledging the considerable gains Germany achieved at the conference table in 1924–25 (well documented in the works of Stephen Schuker, Jacques Bariéty, and Sally Marks), Knipping asserts that France until 1928 still possessed considerable "coercive" and other means to contain its eastern neighbor. But that summer the Weimar government, impatient over the delayed rewards of Locarno, commenced its campaign to liberate Reich soil in the West, reduce reparations, and achieve equality of armaments.

With impressive mastery of German, French, and British archival sources and of the main monographic and theoretical literature, Knipping traces five stages in the unequal duel between Berlin and Paris: the reparations and Young Plan negotiations between September 1928 and June 1929, prerequisite to an early evacuation of the Rhineland; the "zenith of the Locarno Era" between June and September 1929, when Stresemann obtained Allied withdrawal without accepting any additional reparation or security commitments; the hardening of relations between October 1929 and March 1930, marked by the onset of the world depression and significant changes in leadership in Berlin and Paris; the stormy period from April to September 1930 marked by intense revisionist clamor in Germany and bitterness in France; and the "crossroads" between September 1930 and March 1931, when Germany's economic misery and the Nazi challenge incited the government of Heinrich Brüning to more or less terminate cooperation with Paris and choose *Mittleuropa* over *Panuropa*. According to Knipping, the announcement of the Austro-German customs union signaled the end of the Locarno era, actualizing France's fears of another war.

Knipping explains Weimar Germany's obsessive campaign to liquidate the Versailles treaty's economic,

military, and territorial clauses and France's feeble defense of the status quo in structural terms, as reflecting a "generalized fragmentation of the international system" after 1919 (p. 227). At issue was Germany's identity in Europe and the world, either as a "perpetually controlled, middling Great Power" or as the revived "semi-hegemonic Bismarckian state" (p. 9). With judicious language and remarkably balanced treatment, Knipping has detailed and analyzed the main issues and protagonists, even-handedly assessing the hard-liners behind Brüning and Briand. He has paid due attention to the competition for British and American support, particularly on disarmament and debt questions, and he has shown how Germany, despite the ravages of the depression, consistently evaded France's politically laden overtures for bilateral economic and financial accords. To be sure, the Locarno partnership was also strained by Germany's incessant revisionism against France's ally, Poland, to which Knipping only alludes; Stresemann's cautious, and his successor Julius Curtius's aggressive, support of the German minority in Poland between 1928 and 1931 undoubtedly contributed to the demise of Franco-German détente.

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R. H. HAIGH *et al.* *German-Soviet Relations in the Weimar Era: Friendship from Necessity*. Totowa, N.J.: Barnes and Noble. 1985. Pp. viii, 206. \$34.95.

The authors have written this book as part of a larger study of the origins of the Nazi-Soviet pact. Thus far, in addition to the present work, they have published *The Years of Triumph? German Diplomatic and Military Policy, 1933–1941* (1986) and *Soviet Foreign Policy, the League of Nations and Europe, 1917–1939* (1986). The slim volume under review here is composed of 147 pages of text and appears to be intended as a brief introduction to the subject for undergraduates. It is based almost entirely on secondary accounts written in English. The authors' research is dated as well. Most of the works cited were published before 1970, and several important recent studies of Russo-German relations are absent from both the notes and the bibliography. For example, on the German side, the authors have used neither Gaines Post's important book, *The Civil-Military Fabric of Weimar Foreign Policy* (1973), nor Werner Angress's *Stillborn Revolution* (1963). On the Soviet side, they have not used such major works as Richard Debo's comprehensive *Revolution and Survival* (1979).

The authors' writing style is problematic as well. Large parts of this book are constructed by patching together one quotation after another from secondary works. Including the key paragraph from a diplomatic note or an occasional pithy quotation from George Kennan can spur the reader's interest, but the authors' excessive reliance on quoting the opinions of others

threatens to submerge their own analysis and exposition. Their use of Herbert von Dirksen's racist interpretation of German-Polish relations is a case in point.

Focus and scope are problematic, too. The authors apparently intended to write a comprehensive, if compact, survey of Soviet-German relations from 1917 to 1933. Quite properly, their discussion of international affairs is interwoven with a description of the German domestic situation. Unfortunately, very little analysis of the interrelationship of Soviet internal and foreign affairs is attempted. The coverage is incomplete even for the German scene. The authors stress the "Eastern" orientation of post-World War I German policies but underestimate the strength of the countervailing pro-Western tendencies. They have little to say, for instance, about the policies of the Social Democrats. They have also confined themselves mainly to the diplomatic level of Russo-German relations. The interactions of the Soviet and German Communist parties are largely ignored.

Specialists will learn nothing from this work, and students new to the subject would be better served by works based on thorough research and a more comprehensive approach to the German-Soviet relationship.

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H. STUART HUGHES. *Sophisticated Rebels: The Political Culture of European Dissent, 1968–1987*. (Studies in Cultural History.) Cambridge: Harvard University Press. 1988. Pp. 172. \$20.00.

In 1958 H. Stuart Hughes published *Consciousness and Society: The Reorientation of European Social Thought, 1890–1930*, a now-classic book in intellectual history that defined a subgenre of "social thought" for a generation of Hughes's students. Thirty years later, in *Sophisticated Rebels*, Hughes openly admits the failure of his category to illuminate the period from 1968 to 1988: "the very notion of social thought was bound up with a situation in the practical world and a role for the intellectual within that world which we no longer recognize as our own" (pp. 1–2). The problem for the intellectual historian of the recent past, then, is grave: how to conceptualize an unfamiliar territory lacking stable disciplinary supports.

Hughes must be given credit for the effort he makes to compose an intelligible narrative and analysis out of the inchoate materials of the past two decades of European intellectual history and to do so when his customary strategy of interpretation fails him. In place of "social thought," Hughes inventively turns to "political culture" as his Ariadne's thread. Admittedly the term "political culture" is never clearly defined in the book. In lieu of theoretical clarification, Hughes selects from among the intellectual, cultural, and political leaders and movements of the period those that he

regards as "sophisticated rebels," figures and groups who somehow deserve our attention. In elegant prose he takes the reader on a fascinating tour of the events in France during May 1968 and in Prague during 1968; the works of Michel Tournier and Milan Kundera; the fate of guest workers and ethnic minorities; the politics of Pope John Paul II, Poland's Solidarity, Germany's Greens, Soviet dissidents, and social democracy. Hughes's urbane, sympathetic intelligence informs and delights the reader as the often-sad panorama of European political culture unfolds in his pages.

Intellectual historians will be surprised that Hughes dismisses French poststructuralism in a couple of badly misinformed pages. Jean-François Lyotard, for instance, is misleadingly described as "Derrida's heir" (p. 146). Of Michel Foucault's corpus only *The Order of Things: Archeology of Human Sciences* (1973) is discussed, and then merely in a short paragraph. Jacques Derrida is dismissed in two sentences; Gilles Deleuze and Jean Baudrillard are not mentioned; Jacques Lacan is only mentioned; Roland Barthes of *Mythologies* (1957) is treated in passing. Many of those thinkers are inappropriately labeled "structuralists," and little effort is made to account for their tremendous impact on humanists and social scientists in the United States.

German intellectuals are equally omitted. Nothing is said of the important work of Theodor Adorno. Niklas Luhmann is totally absent. Jürgen Habermas is sympathetically addressed for his political stance as a defender of social democracy. Yet, among his theoretical works, only *Knowledge and Human Interests* (1971) is mentioned. His magnum opus of 1984, *The Theory of Communicative Action*, is ignored.

Because one may argue that the "sophisticated rebels" in "social thought" are the thinkers named here and that they continue to flourish in France and Germany, the reader is left to wonder if the subgenre of "social thought" is indeed an extinct cognitive species and why Hughes has chosen to contribute to such a ravaging process of cultural ecology.

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ALAN MACFARLANE. *The Culture of Capitalism*. New York: Basil Blackwell. 1987. Pp. xviii, 254. \$29.95.

In 1978 Alan Macfarlane's *The Origins of English Individualism* appeared, stirring up more controversy than has any other recent work on English history. On its surface, the thesis of that book seemed curiously old-fashioned: English society was different, fundamentally different, and had been different since at least the late thirteenth century; English society was different not just from the rest of the wider world, with which Macfarlane as an anthropologist had been initially concerned, but from most of the rest of Europe;

England was not, had possibly never been, a peasant nation.

This book is a collection of essays with a postscript and an appendix. The essays have all appeared under other covers; all postdate the onset of controversy over the 1978 volume; all speak to that book's critical reception (the postscript, which lines up the work's reviewers for severe correction, most obviously). The essays, which are presented in the order in which they were written and are largely unchanged in form, concern peasants ("The Peasantry in England before the Industrial Revolution: A Mythical Model?"), population, nature ("Man and the Natural World"), violence ("Peasants and Bandits"), evil, love, capitalism, and revolution—that last the most ironic heading, for the essay's thesis is the inapplicability of the concept of revolution to the English experience.

Macfarlane has fun with the notion of decorous, earthbound English witches in the chapter on evil and plays at Eric Hobsbawm's expense, noting that the only English bandits were a romanticized highway robber (Dick Turpin) and a legend (Robin Hood) in Hobsbawm's argument that England was a nonviolent society, with economic rationality (getting rich more quickly) lying at the base of most violence. One of the disquieting aspects of both this book and the earlier volume is the ease with which Macfarlane's arguments about English law and English society can be made into a defense of restrictions on English liberty in the face of Celtic (that is, non-English) political violence.

We return with him to the familiar ground of Kirkby Lonsdale and Earls Colne, raising again the difficulty of knowing how representative of early modern English society these parishes of relatively remote Cumbria and London-orbiting Essex were. Those who have studied the Midlands have done so from a very different perspective; Macfarlane cannot continue to have it both ways, criticizing W. G. Hoskins and his students on the one hand and relying on them for information on this large open-field region on the other. The notion of underlying long-term continuity in England sits comfortably with much recent work that Macfarlane does not discuss, especially with the banishment of "Industrial Revolution" from the vocabulary of English economic history, as his treatment of controlled fertility as a choice between children and other goods has its fuller expression in the theories of Gary Becker.

Macfarlane stresses the too facile conflation of evidence pertaining to banditry and to the late development of companionate marriage—evidence from the cradles of continental European peasant culture combined with thin English examples. One is left, however, with the question of how much of the Continent also did not fit the model of peasant culture. He refers to the Low Countries, northern France, and Scandinavia as possible exceptions. To what extent did they share in the culture of capitalism? And what, after all, were the origins of English individualism?

It is good to have these essays collected in one volume, to judge how Macfarlane has responded to

criticism and has expanded on his original treatment. The book cannot help but form a blue-jacketed companion to the redcoat original. But, because the essays were written independently of each other, while his thought was so focused on that one volume, the collection as a whole is necessarily repetitive, and repetitively defensive. Repetition was inevitable: each essay has stood alone, yet all can be traced back to a single root in *Origins*. Defensiveness, although understandable, would have been wearing in a longer volume.

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DAVID LEVINE. *Reproducing Families: The Political Economy of English Population History*. (Themes in the Social Sciences.) New York: Cambridge University Press. 1987. Pp. ix, 251. Cloth \$34.50, paper \$9.95.

As English historians have become acquainted with the work of their colleagues in historical demography, they have become increasingly interested in the connections between population trends and social and economic structures. David Levine's foray in this book into the battle over the nexus between production and reproduction is an example of the historian's fascination with demography. Indeed, this volume is not the first product of Levine's interest; in 1977 he published *Family Formation in an Age of Nascent Capitalism*, a study of the demographic and occupational structure of two Leicestershire villages from the seventeenth to the nineteenth centuries. But in his new work Levine is far bolder, offering theoretical models of population structures and tackling the *longue durée* by attempting to explain how and why reproductive strategies sometimes created and oftentimes responded to economic forces from postconquest Britain to early in the present century.

Although the book is stimulating and asks a number of important questions, two faults are apparent. The first is that as a historian of the modern age Levine glosses over the subtleties, nuances, debates, and controversies in medieval and early modern English history to such an extent that his picture of premodern England is scarcely recognizable to the specialist. At times it appears that what Levine perceives as change in economic structures over time is the result of misunderstandings of what preceded. Second, and perhaps more significant, Levine has not thought through carefully enough the important questions that he has posed. Although he claims in his preface to be thinking out loud, I imagine that Levine believes that he has clearly determined the forces that effect and affect population movement.

Levine begins by conjuring a picture of medieval England dominated by what he calls a "cottage economy" (pp. 19–21). Associated with this economic structure is a particular family formation system and demographic order, the "peasant demographic model," with controlled fertility and relatively high mortality

(pp. 82–86). Sometime in the course of the sixteenth through the eighteenth centuries the peasant model gave way to a “proletarian demographic model” as the “cottage economy” declined and proto-industrialization increasingly had an impact on the rural economy. Population increased but not at as rapid a rate as Levine believes one should expect given the demand for surplus labor.

Levine endeavors to explain his self-generated dilemma. Although he accepts the reconstruction by E. A. Wrigley and R. S. Schofield of demographic trends after 1541 and endorses their conclusion that population growth was driven by higher fertility occasioned by earlier age at marriage for females (a position that they have since modified), he disagrees that the demographic trend can be explained by increases in real wages. Levine's explanation is much more complicated and is related to alterations in how family formation strategies responded to economic forces. Levine offers a periodization of demographic change in which proto-industrialization moves toward full industrialization in the late nineteenth and early twentieth centuries. Having created a number of time spans, Levine finds that his chronology does not actually fit observed demographic behavior. Part of the problem is that he uses various demographic measures (sometimes growth rate, sometimes reproduction rate, sometimes gross reproduction rate) at different times in his analysis. Ultimately, though, his attempt to distance himself from the Wrigley-Schofield explanation fails. After all, it is possible that family formation and the rate of industrialization were both affected by the trends in real wages.

There is a tendency in our trade to be overly critical of essays that have grandiose designs. Levine asks an array of interesting and important questions about the relationship between people and resources as well as the way in which individuals cope with change. His ideas will prompt useful debate and should stimulate the type of research into economic and social structures that would allow a historian of equal daring to write a more definitive essay a generation hence about the political economy of English population history.

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LYNN F. PEARSON. *The Architectural and Social History of Cooperative Living*. New York: St. Martin's. 1988. Pp. x, 274. \$39.95.

English domestic architecture, especially the familiar terraced house, has long been regarded as one of England's most significant contributions to the organization of everyday life. Indeed, continental observers at the turn of the twentieth century were often impressed with the aptitude of the English for creating congenial living surroundings. The English themselves, however, were more critical. Much of the housing stock, they observed, consisted of squalid “back to back” houses,

overcrowded and decaying terraces, and horrible cellar dwellings, and even the housing in better condition often lacked the amenities to make housekeeping either pleasant or efficient. Cooperative housing was offered as an alternative. Between 1874 and 1925 fifteen cooperative housekeeping developments were begun; all of them incorporated not only new domestic architecture but also alternatives to the conventional organization of housework.

In this detailed study, Lynn F. Pearson traces the history of cooperative housekeeping in England and considers its ideological underpinnings, its architectural manifestations, and its limited and transient success. Among the widely varied motives for advocating cooperative housekeeping were utopian communalism, feminism, and philanthropy. Also involved was a desire to improve housing, reduce the labor of housewives, and increase privacy by eliminating paid help in one's home. The author explores these ill-assorted and sometimes conflicting objectives and, with the aid of floorplans and photographs, describes the material forms they took. Almost all of these forms addressed the requirements of cooperative living in a characteristically English fashion by ensuring that a significant amount of personal privacy coexisted with communal activities and by never straying far from traditional architectural styles. Indeed, the new housing designs would have been familiar to any Oxbridge undergraduate, and the exterior facade of even the most innovative living arrangement was comfortably conservative.

Of all the experiments in cooperative living, the most successful were those that provided housing for single working women. This kind of cooperative housing thrived because it filled a genuine gap in the accommodation market; reasonably priced housing of decent quality was in short supply for self-supporting women. Cooperative housing replaced often-squalid, single rooms with accommodations that met some of the social as well as physical needs of women living alone, and it required no significant alteration in household organization.

The needs of families, in contrast, were much less successfully met through cooperative housing. The flats that dominated this form of accommodation were never accepted as appropriate dwellings for children and were too readily associated with the grim tenements erected for the poor. Moreover, communal cooking, washing, and other joint activities seemed an intrusion into the fabric of family life. Concerns about “forced communality and charity,” noises, cooking smells, and unsupervised servants, and the fact that “cooperation” did nothing to alter the sexual division of domestic labor, combined to defeat those experiments. Ultimately, it was takeout food and inexpensive restaurants that met many of the needs that the cooperative experiments had set out to address.

In evaluating these experiments in the design and organization of domestic space, Pearson concludes that their influence on subsequent cooperative living and designs in domestic architecture far outweighed their

limited numbers. By assigning household functions priority in the architectural design process, they created precedents for later efforts to mesh domestic architecture with modern lifestyles.

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GRAHAM MAYHEW. *Tudor Rye*. Brighton: University of Sussex Centre for Continuing Education. 1987. Pp. 353. Cloth £19.95, paper £9.95.

Compared to the great abundance of county histories, relatively few studies of Tudor towns have appeared, and those towns examined thus far consist mostly of cathedral cities, county towns, and regional centers, usually possessing economies based largely on cloth manufacturing. Rye, the wealthiest and most populous south-coast port until well into the sixteenth century, depended solely on fishing and trade. Like so many other towns, Rye experienced an urban crisis in the form of severe demographic decline and a sudden withering of trade from which it never recovered and which reduced it to the status of a market town and fishing port. By 1620 its population was one-quarter the size it had been fifty years earlier.

Graham Mayhew has made good use of the extraordinarily rich municipal archives of Rye to demonstrate quantitatively that Rye suffered mortality rates that were much higher than those recorded by E. A. Wrigley and R. S. Schofield in *The Population History of England, 1541–1871* (1981) but that are comparable to those uncovered in recent studies of Worcester and York. Tudor towns were hard put to defend their autonomy against interference by crown officials and local gentry, and it comes as no surprise that Rye's decline hastened its loss of parliamentary independence. We also learn that a mortality crisis in the 1550s may have tipped the balance in favor of the Protestant faction in Rye by carrying off a number of traditionalists among the town magistrates. Considering the thoroughness with which Mayhew treats topics such as the demographic structure and the distribution of wealth and poverty, it is curious that, in a community where three-quarters of all children lost their fathers, he omits discussion of the jurisdiction over orphans, which figured so prominently in the life of other urban communities.

This book is a valuable contribution to early modern urban and social history. The many tables and minute detail will repay study by specialists, but the more casual reader may be forgiven for wondering whether the author cut anything out of his manuscript. Moreover, the editing and proofreading leave much to be desired. Despite a long errata slip pasted in the front of the book, far too many spelling errors, typographical errors, and instances of eccentric punctuation have escaped correction.

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LACEY BALDWIN SMITH. *Treason in Tudor England: Politics and Paranoia*. Princeton: Princeton University Press. 1986. Pp. 342. \$25.00.

The conspiracy theory of history is eternal, but it flourished particularly in the sixteenth century, producing the paranoia to which Lacey Baldwin Smith's title refers. In a world where Christian eschatology competed with the Wheel of Fortune, and scientific understanding of causality was in its infancy, the conspiracy theory provided comprehensible explanations for a whole range of human behavior. The proposition that "the heart of man" is utterly corrupt was also one that appealed particularly to Protestant theologians for whom the free gift of God's grace was the beginning rather than the end of godly and responsible action. Renaissance Europe generally was prone to theories of cosmic subversion, and the scapegoats included Jews, witches, heretics, and infidels, according to taste and location. Not only did society identify and pursue such deviants as a means of relieving anxieties, but deviants also identified themselves in orgies of confession that were seldom the direct result of torture or the fear of it. Like some other historical periods (including our own), the Renaissance suffered from a powerful sense of moral and physical decay, which was one reason why Martin Luther and his followers enjoyed such conspicuous success. As a result, propitiation rituals assumed great importance, and, although the victims of pogroms, autos-da-fé, and treason trials might protest their own innocence, they very seldom questioned the necessity for the process itself. Given orthodox assumptions about the nature of evil, such events signified the will of the community to maintain its place in an ordered universe.

The deviation that obsessed the English was treason, and it is significant that, with only minor exceptions, the author's examples are taken from the second half of the sixteenth century. Before Henry VIII broke with Rome, treason was the same personal and dynastic challenge to the monarch that it had been in earlier centuries. The earl of Warwick, the earl of Suffolk, and the duke of Buckingham died because they were perceived as threats to the Tudor succession. It was only after 1535, and particularly after the settlement of 1559, that treason came to be seen as a force threatening the ideological integrity of England. This was an idea specifically encouraged by Elizabeth and was the reverse side of the coin on which she appeared as the personification of the realm. Smith is particularly convincing as he demonstrates the ways in which the moral dimensions of the crime were identified and explored. The threat to order was both spiritual and social—"untune that string, and hark what discord follows." The conspirator, the false friend, and the domestic traitor were conceptual and literary stereotypes, expressive of the peculiar anxieties of a society that had consciously embarked on innovative courses. If this was paranoia, however, it was not a spontaneous cultural evolution but a deliberate political artifact. In accor-

dance with his own priorities, the author pursues the intellectual and psychological roots of the fears that treason aroused, but he somewhat neglects the objective political basis. It was hardly paranoid to fear the Spaniard in the 1580s or Roman Catholic subversion after 1570. The seriousness of the threat is sufficient to explain why so many trivial incidents were magnified out of proportion. Not all traitors were as ludicrously incompetent as Smith claims, but all sorts of ludicrous incompetents were branded as traitors. Perhaps the only genuine case of paranoia was Robert Devereux, earl of Essex, and a substantial part of the book is devoted to him. This section is a fascinating and excellent biographical essay and is much the best part of an interesting, but not always convincing book. The English feared treason for the same reason that the Spanish feared heresy; treason was a threat not merely to a particular political order but to the whole standing of the community with its God.

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G. J. R. PARRY. *A Protestant Vision: William Harrison and the Reformation of Elizabethan England*. (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1987. Pp. ix, 348. \$49.50.

In this detailed monograph G. J. R. Parry draws deeply from a lengthy manuscript entitled the "Great English Chronology" by William Harrison to reconstruct "A Protestant Vision of History" and "A Protestant Vision of England" put forward in the reign of Elizabeth I by a clergyman hitherto best known for his "Description of Britain" published in the *Chronicles* of Raphael Holinshed in 1577 and 1587. Drawing systematically from Harrison's works and related publications by English and continental Protestants, Parry presents his research in seven chapters: "The Two Churches," "A Reformed Chronology—Patterns and Parallels," "A Reformed Chronology—Interpreting the Prophecies," "A Reformed Church," "A Reformed Prince," "A Reformed Commonwealth," and "A Reformed Natural Philosophy." As well as providing a thorough account of Harrison's interpretations, Parry makes a number of telling points on such general topics as Elizabethan Puritans, prophetic witness, apocalyptic visions, hermeticism, interpretations of history, and the Weber thesis.

The first three chapters set out the structure of Harrison's apocalyptic, historical vision, an interpretation of a wide range of human events as a struggle between the true church of God (the "covenant line," which extended from Adam through the ancient Jews to Elizabethan Reformed Christians) and the false church of Satan, Cain, and Antichrist. Building on the biblical and historical insights of Philip Melancthon, John Bale, and John Foxe, Harrison worked out his

own detailed chronology and interpretations. Prophecies both as predictions of the future and as utterances of inspired instruments of God informed the uniform patterns imposed on historical events: "Any periodisation which did not originate in what Harrison regarded as Elect prophecies, or which did not conform to the knowledge transmitted by the covenant line, had to be rejected" (p. 96). Although skeptical of the complex numerology of the Cabala and Jean Bodin's *Methodus ad facilem historiarum cognitionem* (1566 ff.), Harrison was so eager "to emphasize the divine mystery behind" numerical patterns that he "distorted the evidence" (p. 117). Like many contemporaries, he included portions of the "ancient prophecies" attributed to the Sibyl and Merlin where these seemed appropriate, but his vision remained firmly fixed in a scriptural base.

A supporter of further reformation in what Parry calls the "Grindalian" tradition, Harrison wished to purge the Church of England of what he saw as "gentile" elements. With good enough connections to William Brooke, the tenth Lord Cobham, to obtain several respectable livings, including a prebend of Windsor, he defended pluralism as the only way to bring Protestant preaching to large numbers of people. Sympathetic to the Presbyterians, he showed a practical spirit of accommodation by arguing that, "until the Church returned to that apostolic state of harmony which Satan had subverted in the first century A.D., a pastoral episcopate represented an acceptable alternative to religious anarchy" (p. 178). Disregarding available traditions of basing governance on the law of nature, he turned to the Old Testament as the source of models of civil polity. As Parry demonstrates at length, Harrison saw reason as subordinate to the revelation of God and used his interpretations of scripture (especially of the model society created by God for his people, the ancient Jews) as the test for all human understanding. Although this monograph makes a number of valuable contributions to our understanding of Elizabethan Puritans, apocalyptic thought, historical understanding, and the relationship of English to continental reformers, most readers will find it too densely packed with details and comparisons to sustain their prolonged interest.

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MARTIN INGRAM. *Church Courts, Sex, and Marriage in England, 1570–1640*. (Past and Present Publications.) New York: Cambridge University Press. 1987. Pp. xiii, 412. \$54.50.

Since the early 1970s, the records of the English church courts have become of increasing importance to historians in two quite distinct ways. Several important institutional studies have been based on the records. In addition, social historians have made extensive use of court cases to explore popular attitudes and behavior.

An understanding of court procedures is of little use, however, if we do not know how people actually used the courts, and information culled from cases is quite misleading when removed from its institutional context. By integrating both approaches, Martin Ingram has produced a splendid book that is of great importance to all historians of early modern England.

Focusing on Wiltshire, with special attention to the parishes of Keevil and Wylle, Ingram argues that the church courts' reputation as archaic and ineffective is undeserved. Although he cannot prove that the Wiltshire courts' energetic prosecution of sexual irregularity (particularly in cases of bastardy) resulted directly in increased premarital chastity and marital stability, he is convinced that the courts were vehicles of an important moral reformation.

Ingram's revisionism is based on meticulous research and convincing argument. He is a master of the complexities of church law and procedure but also has much to say about popular religiosity, which he finds remarkably conformist in Wiltshire. Key indicators are the increased usage of church marriage and the decline in the older practice of binding spousals. And, although Ingram's concern with the social history of marriage and sexuality is admittedly secondary, the book is rich with insight into a wide range of unresolved questions. Ingram's exploration of courtship and marriage practice reveals the ambiguity of relations between parents and children that neither Lawrence Stone's emphasis on patriarchy nor Alan Macfarlane's insistence on individualism fully comprehends. It was precisely the contradiction between the permission to love and dependence on parental favor that caused many to come to the church courts for resolution of their personal dilemmas.

It would be wrong, however, to evaluate the courts' effectiveness solely in practical terms, something Ingram, like most other historians, tends to do. Although some people found concrete redress through the courts, many of the personal problems brought forward were in fact irresolvable, being the result of the contradictory values of personal freedom and obedience to family authority already built into English culture. It could be argued that it was in helping people work through ambiguity at the symbolic level that the courts offered their greatest, though unacknowledged, service. As Ingram shows in his superb chapter on defamation cases, the courts were of particular value to married women bent on defending what was becoming their most valuable asset, namely their sexual reputations. But the plaintiffs arrived at court only after all other measures, symbolic as well as practical, had failed. The author is right in seeing the courts as an indispensable part of the social process in early modern England, but greater attention to cultural contradictions would have added yet another dimension to his rich analysis.

Given the vitality of the courts before the Civil War, one wonders how English society could have done without them in subsequent periods. Ingram suggests

that people turned increasingly to the civil courts and the parish during the Restoration, but those never really provided popular access to real and symbolic resources once available through the church courts. During the eighteenth century, the poor were forced to turn elsewhere, which is perhaps one of the reasons why religious conformity, especially in marriage matters, began to crumble. A study of that transitional period, using the records of both civil and ecclesiastical law, would be a fitting tribute to Ingram's splendid example of how historians can, and must, reintegrate institutional and social history.

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ROY PORTER. *Mind-Forg'd Manacles: A History of Madness in England from the Restoration to the Regency*. Cambridge: Harvard University Press. 1987. Pp. xii, 412. \$38.50.

In 1757, Samuel Wiggett submitted his resignation as a trustee of Bethel Hospital, a lunatic asylum in Norwich. "My Spirits are exceedingly Weak," he explained to his colleagues, "I cannot bear to hear and see those melancholy Objects." Roy Porter suffers from no such faintheartedness. His history of insanity in eighteenth-century England revels in the eccentric, celebrating with irresistible verve the fabulous delusions of characters such as Goodwin Wharton, who developed a sexual obsession with a woman whom he thought would help him to enter the orgiastic world of the fairies, and Alexander Cruden, the biblical scholar who fancied himself "Alexander the Corrector" and sallied forth on London's streets to conquer vice.

Porter narrates the stories of mad folk with a sure and engaging touch; he is one of the few historians writing today whom one can justly call a man of letters. This book, let it be said at once, is delightful reading. But Porter aspires to more than brilliant storytelling; he wants to set the record straight, to correct the misapprehensions about insanity and its treatment in Georgian England that earlier writers have propagated. The most sweeping errors were invented by Michel Foucault, and Porter has little difficulty in showing that the "Great Confinement" that Foucault declared to have been the essence of Enlightenment responses to madness did not actually occur. More broadly, Porter attacks the whole school of social historians who have depicted the rise of the asylum and of moral therapy as a means of disciplining the poor and inculcating capitalist values in the recalcitrant. Perhaps the most important argument in the book is that the asylum had its origins in the commercial revolution of the eighteenth century. Most early asylums were small private madhouses, catering to paying customers. Their inmates were not the laboring poor but the lunatic relations of the propertied elite. His adjustment to the revisionist picture of moral management and moral therapy is less drastic, but it stresses vital themes

that Foucault and his followers chose to ignore in their anxiety to depict the rise of psychiatry as a conspiracy of the capitalist ruling classes.

Porter rightly (but somewhat inconsistently) insists that historians who have viewed Georgian asylums and treatment of patients as a disaster for the insane have overlooked evidence of humane conditions and sincere efforts to restore the insane to health and happiness. No doubt the picture has been drawn in tones too dark; the more we learn, the less easy it is to make bold and simple generalizations. He is also emphatically correct to see the secularization of ideas about insanity and treatments of the insane—a theme entirely neglected by Foucault—as a dominant theme in the history of madness in the eighteenth century.

Although this book thus corrects some of the most often-repeated and exaggerated claims of Foucault, Klaus Doerner, and other influential figures, it is not without its own faults. Porter neglects manuscript evidence almost entirely, although many of the most important sources are unpublished. He fails to analyze in adequate detail the institutions that cared for the insane—the public and private asylums, local courts and benches of justices, and the court of chancery. So superficial is his research on the last, for example, that he presents the chancery as the lunatic's friend, when in fact the court was notoriously corrupt, costly, and inefficient. There are other, similar lapses. Porter writes as a scholar perched on the shoulders of dwarfs; his references to his predecessors dwell on the foolishness of their judgments and the incompleteness of their research. He may well be right about their failings, but, when one sizes up this study, it turns out to be less imposing than we might have hoped.

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TIM HARRIS. *London Crowds in the Reign of Charles II: Propaganda and Politics from the Restoration until the Exclusion Crisis*. (Cambridge Studies in Early Modern British History, number 2.) New York: Cambridge University Press. 1987. Pp. xiv, 264. \$42.50.

London politics has always been crucial in English history. London, it was said, led the dance and the rest of the country followed. Tim Harris, a highly talented young scholar, examines the London scene at its widest, including Westminster, Southwark, and parts of Middlesex. And he does not concentrate purely on the role of the city corporation or of the electorate, examining the part played by the crowd and the ways used to shape opinion by ritualistic demonstrations, newspapers, pamphlets, ballads, plays, playing cards, and coffee-house talk. He disagrees with assumptions made by other historians that the masses were apolitical and incapable of coordinating direct popular actions themselves. The London crowd was composed not of society's dregs but of respectable people of lowly status,

well able to defend the legitimacy of their actions by broad notions. Nor was its standpoint static. Broadly speaking, the London crowd was pro-Stuart in 1660, Whig in 1679–81, anti-Stuart in 1688, Tory in 1710, anti-Hanoverian if not unambiguously Jacobite after 1714, and pro-Wilkes in the 1760s. It was not monolithic, however, for there were separate Whig and Tory crowds, so that popular culture was not consensual. We are presented with a skillful blend of political and social history, taking aboard recent work on the role of the electorate and public opinion as well as revisionist views on the importance of religion in politics. Economic factors are analyzed, but it was not material hardship, it seems, that drove people into the hands of the Whigs.

It was during the Exclusion crisis of 1679–81, a contemporary noted, that what used to be called the “rabble” became the “mob.” To influence common people, the Whigs applied skillful propaganda techniques, playing on old paranoid fears of popery and new fears of French tyranny. The vast majority believed in the Popish Plot, and it was thought feasible that about twenty thousand Roman Catholics would exterminate about one hundred thousand Protestants. In Maryland, it was reported, papists were using Indians to kill Protestants. The religious persecution of Nonconformists, which affected the lower orders much more severely than the elite, fueled Whig agitation. In the great pope-burning ceremonies of the Exclusion crisis, it was not only the pope but bishops of indeterminate denomination who were burnt in effigy. Then the heroes were Titus Oates, the duke of Monmouth (Charles II's bastard son), and the earl of Shaftesbury.

The great weakness of the Exclusionists, however, was that they had no credible successor with which to replace James, duke of York, a Roman Catholic. By 1680 the public mood began to change. Fears of the Anglican majority about any breach in the hereditary succession to the crown were compounded by fears for the safety of the Church of England. Tory propaganda, which was orchestrated by government propagandists such as Roger l'Estrange, ignored the question of the duke of York's religion, arguing instead that by splitting the established church the Nonconformists were playing the game of the Church of Rome. These views aroused genuine popular backing. London apprentices assured Charles II that they would support no breach in the royal succession, and at popular demonstrations it was the turn of Jack Presbyter and Oliver Cromwell to be burnt in effigy. Harris concludes that London was a divided society, much as Gary De Krey has shown it to be a fractured society after 1688.

Harris is to be congratulated in producing a lively and very readable book. His publishers have given him lavish illustrations, and it is a pity that some are too blurred or on too small a scale to make their point.

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JOHN BARRELL. *The Political Theory of Painting from Reynolds to Hazlitt: "The Body of the Public."* New Haven: Yale University Press. 1986. Pp. viii, 366. \$30.00.

This is John Barrell's fourth and—to my mind—his most impressive book. It arrives at a time when scholars are paying increasing attention to the relationships between political theory and social history, on the one hand, and works of the "imagination," on the other. But, if we now share a heightened sense that these connections are important, then we have only begun to catch up with Barrell, who began his career by examining how "the sense of place" in John Clare's poetry reflected both landscape theory and social change and then wrote two provocative books about the poor in English landscape painting and the quest for a unified view of English society in the work of James Thomson, John Dyer, Samuel Johnson, and Tobias Smollett.

In many ways this book is closely tied to the concerns of Barrell's *English Literature in History, 1730–80: An Equal, Wide Survey* (1983) in which he asked "how some writers of the period themselves attempted to construct an understanding of contemporary social changes" (p. 13). Barrell's answer is that, even when writers could convince themselves that society was fundamentally coherent, they still encountered difficulties in determining the "viewpoint"—the intellectual or social position—from which this coherence could be observed and the language in which it could be described (p. 177). In the course of his examination of Johnson's *Dictionary*, Barrell noted the analogies that were frequently made between the English language and the English constitution; both, for example, were founded on principles of "freedom" and "consent."

A similar relationship lies at the heart of *The Political Theory of Painting*: "In the early decades of the eighteenth century in England, the most influential attempts to provide the practice of painting with a theory were those which adopted the terms of value of the discourse we now describe as civic humanism. The republic of the fine arts was understood to be structured as a political republic; the most dignified function to which painting could aspire was the promotion of the public virtues; and the genres of painting were ranked according to their tendency to promote them" (p. 1). Building on the work of J. G. A. Pocock, Isaac Kramnick, and Donald Hanson, Barrell reveals how this theory was "attenuated" during the course of the eighteenth and early nineteenth centuries. Jonathan Richardson, among others, argued for an expanded definition of the public that would include the artists themselves. Joshua Reynolds, the major figure in Barrell's study, attempted to ground public spirit on social knowledge rather than virtue; he replaced the rhetorical with a philosophical aesthetic and struggled to show what our individual characters have in common. James Barry believed that painting "should depict the diversity of a society divided by the division of labor, but in such a way as enables its members to grasp the common ends, political and religious, which their di-

vided labours co-operate to achieve" (p. 63). Henry Fuseli spoke in two voices, sometimes reaffirming the most traditional form of civic discourse, at other times adapting it to a dramatically changed social environment. Benjamin Haydon, Barrell concludes, was the last theorist to defend the preeminence of the grand style in civic terms and William Hazlitt the first to argue that the ends of painting were primarily private.

Throughout his analysis Barrell is concerned to show that those who wrote about painting were not simply borrowing the discourse of civic humanism for their own ends. As Pocock has pointed out, "The dominant paradigm for the individual inhabiting the world of value was that of civic man" in the early eighteenth century, and Richardson and Reynolds, in particular, were anxious to demonstrate how the subject matter of painting was able to raise it from a mechanical to a truly liberal art. It is, in fact, "their concern to understand the history and function of painting in terms of its relation to the political" that Barrell finds of value in their work (p. 341).

I doubt whether anyone who has the patience to work through Barrell's taxing book will ever read eighteenth-century aesthetic theory—and especially Reynolds's *Discourses*—in quite the same way again. At the same time, scrupulously objective as this study is, it nevertheless evades two issues that will surely occur to many of its readers. Most of the writers Barrell examines were better known as painters, yet their work in the studio receives virtually no attention. How, for example, does Reynolds's adherence to a revised form of civic humanism square with his work as a painter of portraits, and is his vision (or that of Richardson or Godfrey Kneller) "attenuated" in the canvases of Barry, Fuseli, and William Blake? No one will fault Barrell for not pursuing these questions in an already ambitious book, but I find it odd, given his interests in *The Dark Side of the Landscape* (1980), that he decided not to speculate on the direction such an inquiry might take.

My second objection is prompted by Barrell's conception of the historical forces that led to such an attenuation of the discourse of civic humanism. Barrell provides two eloquent summaries (on pages 88 and 339) of how the structure of society had become too complex for individuals to understand their relation to a public and of how a discourse that claimed to be independent of social and economic change was eventually seen to be marginal to modern life. Although I have no reason to doubt these conclusions, I find it unsettling not to have them corroborated by reference to the extensive social history on which Barrell could have drawn.

And yet even theory reinforced by social history may still disregard the significance of other forces that are "political," especially changes in the form and functions of government or of the party system. In this book Barrell's focus is firmly intertextual: "political" should almost always be read as "the ideological concerns that emerge in verbal discourse." I want to question

whether there are not other areas of enquiry, much closer to hand, where we can gauge the persistence of an influential political theory in the life of the painter. One important subject in this context, I would suggest, is the history of the Royal Academy and particularly the troubled relations that existed between the academy and Barry and Haydon (and even Reynolds at the end of his career). The object of such an enquiry would be to analyze what Thomas Crow, in *Painters and Public Life* (1985), has called "a sphere that was at once a discursive formation and a site of actual social practice" (p. 255). Barrell might well resist this line of questioning, but not, I hope, an attempt to suggest how his work might be opened up to the concerns of historians and art historians alike.

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DUDLEY MILES. *Francis Place: The Life of a Remarkable Radical, 1771–1854*. New York: St. Martin's or Harvester, Sussex. 1988. Pp. xi, 303. \$35.00.

It has been nearly a century since Graham Wallas wrote the last full-length study of Francis Place. The lack of interpretations of Place has caused a serious, but understandable, gap in the literature. Place's career and personality at once invite and deter the potential biographer. An important figure in every aspect of London reform politics for more than half a century and the chief chronicler of much of that history, he was also an intemperate and arrogant busybody who left behind a truly daunting mass of often turgidly written archival material. Another reason for the neglect of Place is that his gradualist and pragmatic reformism, which so endeared him to the Fabian Wallas, is anathema to recent historians of the Left, who have sought to resurrect a more radical, even revolutionary tradition against which Place worked throughout his career. Surely it is not entirely accidental that this new biography was written by someone outside the profession.

That it needed writing is, however, undeniable. Place had an extraordinary career; he began that career in the service of the London Corresponding Society after most of its early leaders had been hounded out of public life by savage governmental repression. It was there, perhaps, in the desperate attempt to keep the movement alive, that Place learned to prefer political survival to the noble but futile gesture, a preference that earned him the scorn of the ultra Left, then and ever since. Place played a central role in the revival of radicalism a decade later, through the election of Francis Burdett to Parliament and the founding of the Westminster Committee. There he perfected his considerable skills as a backroom politician. Like most radicals he believed deeply in the need to educate the whole of the population and was one of the founders of both the British and Foreign Schools societies and the mechanics institute movement. His work in education

brought him into contact with the founders of utilitarianism, Jeremy Bentham and James Mill, with whom the scholarly, some might say pedantic, side of Place found its natural home. Reform founded on research became as much his creed as theirs, and, married to his political acumen, it proved a powerful weapon. Place masterminded the campaign for the repeal of the Combination Acts; he dominated the movement in the metropolis in support of the Great Reform Bill; and he did much to shape the character of London Chartism. Thereafter his influence declined, as he grew older and the center of radicalism shifted to the provinces, although he continued as an advisor to other leaders and movements until the end of his life.

In recounting his long and complex career, as in treating Place's private life and often quirky personality, Dudley Miles is both thorough (remarkably so considering the modest length of his book) and never less than fair. Where the book disappoints is in more than occasional lapses in proportion and in a closely related tendency to concentrate so intently on Place as to lose sight of the context in which he operated. For example, metropolitan politics was plagued during this period by *prima donnas*—Place not the least among them—and Miles gives far too much space to the minutiae of personal and factional infighting. Most of this information could have been omitted, but, even where it is important, much of its significance is lost because its roots in the changing character of metropolitan political society are not adequately traced. Miles rightly emphasizes that Place was born in Hogarth's London and died in Victoria's, yet the revolutionary nature of that transformation, its effects on Place, and Place's role in it are hardly mentioned. This is not, in short, the biography of Place that we might have wanted. But, although not inspired, it is, nonetheless, reliable and therefore welcome.

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KEITH ROBBINS. *Nineteenth-Century Britain: Integration and Diversity*. (The Ford Lectures, 1986–87.) New York: Clarendon Press of Oxford University Press. 1988. Pp. 199. \$29.95.

For such a compact geographical and economic area, the United Kingdom contains a rich mixture of nationalities. Political frontiers between them have long disappeared, but psychological barriers remain, and it cannot be asserted that they are diminishing. In the late 1960s and early 1970s there was a stronger upsurge of Scottish and Welsh nationalism than had occurred for centuries. Whether strong nationalist sentiments persist or not, Scots, Welsh, and Northern Irish people within the United Kingdom will doubtless continue to "feel" different from English ones. The minority nationalities, though different from each other, share a "small minority" consciousness, causing assertiveness that may, in turn, help produce notable achievement in

the general British context. Of the eighteen prime ministers in this century, five have been Scots and one Welsh.

There is a formidable amount of published historical work on the different national sections of Britain, but very little gives comparative integrated treatment to Britain as a whole. Keith Robbins's new book is an apt reminder that Britain is more than England and that all countries in Britain have internal regional distinctions. The book is an interesting introduction to a complex subject, but it can be only an introduction. Within the brief compass of the Ford Lectures, Robbins can only survey the main questions and tendencies; the information he gives is sometimes telegraphic and occasionally misleading.

In the nineteenth century, with which the book is mostly concerned, Britain (though not the United Kingdom, on account of the Irish question) became in some ways more integrated than it has been since the mid-1960s. Nonetheless, the nineteenth century displayed the dichotomy of integration and diversity that has been shown more strikingly in recent years. In that century the Welsh were much more Nonconformist than the English and came to differ radically from them in majority political complexion. Welsh was widely and habitually spoken, and many Welshmen did not know English. The Scots, unlike the English, were mostly Presbyterian and, unlike the Welsh, had established ecclesiastical, educational, and judicial systems distinct from those of England. They, too, differed from the English in majority political complexion, and many more spoke Gaelic than do today. But industry prospered in all three countries; economic and social bonds strengthened between them; they were conscious of combining to lead the world's greatest empire; the people were little represented in politics until toward the end of the century; and nationalist expression was weak in both Scotland and Wales in marked contrast to Ireland. In the twentieth century, secularization has strongly affected Britain as a whole; Welsh has become much less generally spoken (though it continues to flourish); and Gaelic is spoken by far fewer, although it, too, survives. But democracy has become fully established; the empire has been abandoned; Wales and Scotland are among the areas most affected by industrial decay and unemployment; and there is abundant oil production off Scottish shores. Those factors explain the development of political nationalism.

Robbins's wide-ranging account spans culture, religion, commerce, education, sport, and patriotism. He shows well the constitutional and cultural differences that maintained diversity yet, when treated sensitively, did not obstruct integration. But the more amorphous psychological territory of nationalism is little explored. This is rather a pity because it is there that the mental roots of diversity are to be found. Institutional equality and official encouragement of cultural distinctiveness cannot eradicate small and inescapable everyday irri-

nants that are taken to exemplify the majority status of the English.

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RUTH RICHARDSON. *Death, Dissection, and the Destitute*. New York: Routledge and Kegan Paul. 1987. Pp. xvii, 426. \$29.95.

In this book, Ruth Richardson presents us with a fine study of the events leading up to and including the passing of the English Anatomy Act of 1832 and some of its repercussions. This volume is a pioneering piece of work, the first major study to be made of the place of dissection in medical culture and education, of grave robbery and reactions to it, popular attitudes toward the corpse and its appropriation by the medical profession, and the passage of the act, which replaced what "had for generations been a feared and hated punishment for murder" with one "for poverty" (p. xv).

The study leads us through some of the fascinating rites, survivals into the nineteenth century, surrounding death and the safe passage of the deceased, both bodily and spiritually, through watching and burial. Discussed in detail are the rising importance of anatomy in the medical curriculum, the difficulties (perceived or real) experienced by the medical profession in obtaining sufficient corpses (officially before 1832 the bodies of hanged murderers), and the increase in grave robbery, which, becoming more remunerative, gave rise to the phenomenon of "burking." Richardson lays to rest the myth that the Burke and Hare affair, and the popular risings that followed it, led directly to the passage of the Anatomy Act, the murders being discovered several months after the first Select Committee on Anatomy had reported in 1828.

Richardson's study provides us, above all, with a meticulous and well-crafted study of the two anatomy bills, an analysis of their framers, and the debate in and outside of Parliament, as the promoters of the bills attempted to solve at one blow the problems of supplying bodies to the anatomy schools and grave robbing. The diverse alternatives raised to the appropriation of the bodies of unclaimed paupers and hospital patients, and their largely summary dismissal, are analyzed, the appropriation of the corpses of the poor being described as "a foregone conclusion" (p. 121). The final section examines some of the act's consequences, the associated corruption and opposition, and problems of administration.

Richardson argues that the passage of the Anatomy Act not only was geared to obtaining an adequate (as defined by the medical profession) supply of bodies for dissection but also came to function as an instrument of control and repression of the poor, largely because of the efforts of Benthamite Members of Parliament and the setting up of the first centrally administered national inspectorate. In this the medical profession, wishing to sever its association with the executioner and

increase the supply of anatomical subjects, concurred and cooperated.

At one or two points Richardson herself seems to lapse into making "foregone conclusions" and has perhaps also underestimated the progress made in balancing the "Great Men of medicine" mode of writing medical history in the last decade. More evidence would have been appreciated to back up some of her statements, such as the conclusion that the much-cited case of Bransby Cooper, the inept nephew of Astley Cooper, and his bungling surgery was typical. The notion that the treatment of the poor by civic authorities and the medical profession in cholera epidemics was uniformly coercive and insensitive surely cannot be fully substantiated. And is the association of the New Poor Law of 1834 and the Anatomy Act with Nazi concentration camps (p. 268) really valid or appropriate?

The author admits to being frustrated by a lack of material (p. xiv), a problem that results in part from the fact that the book deals with very new subjects and sources. Additional local studies, of less publicized communities and events, might have given a more balanced picture. Richardson's case, however, is, on the whole, well argued and well supported. Her argument is balanced by her acknowledgment that certain groups of the medical profession were opposed to the appropriation of the corpses of the unclaimed poor and that some aspects of popular thought, publicized by individuals such as Francis Place and Richard Carlile, had abandoned the significance of the corpse and repugnance to dissection.

Richardson's study represents many years of painstaking and rigorous research into largely new territory. The book is well produced, with detailed bibliography and notes, a useful index, and well-chosen illustrations. Overall, this is an excellent book, a delight to read and an important contribution to nineteenth-century social history and the history of medicine.

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FRANK MORT. *Dangerous Sexualities: Medico-Moral Politics in England since 1830*. New York: Routledge and Kegan Paul. 1987. Pp. ix, 280. \$14.95.

This very interesting book is valuable to the history of sexuality for two reasons. First, it deepens and broadens our knowledge of the way sexuality was regulated in English society between 1830 and the 1920s. Secondly, it provides historians with an unusually astute analysis of the ways certain new methodological innovations may be employed in this field of inquiry.

Frank Mort does well at the second task. He considers how the "discourse analysis" of Michel Foucault may be "historicized," that is, applied to the changing cognitive structures societies have used to understand and manipulate sexual identities. Mort breaks down

sexuality into its component "discourses"—medical, moral, scientific—and shows in convincing detail that sexual identities changed as the relative strength and make-up of each discourse varied over time. He is careful to remind his reader how he is handling his analytical task at each stage of his discussion, and he is sensitive to the historical context in which these discourses were produced. Mort does contest Foucault's notion that power in modern societies may no longer be understood fruitfully as an exercise of authority from above by illustrating how politics and the law helped construct and define the boundaries of the sexually permissible. But he also appreciates how sexual discourses were generated within society itself by specialists and voluntary organizations, sometimes opposing, sometimes supporting, the interests and outlook of the state.

Mort's central point is that all public discussions on issues relating to population, public health, and the family were permeated by linguistic and rhetorical codes that supported particular discourses on normal and abnormal sexual behavior, masculinity and femininity, and love. Policy decisions, and the activities of voluntary organizations and "purity" groups, followed closely the prescriptive messages of those codes. Mort identifies three fundamental shifts in the dominant sexual discourse between 1830 and 1920, each of which had immediate implications for policy and social action. The first, moral environmentalism, was reliant on an environmentalist medical discourse that sought to improve the welfare of the population by striking a balance between state regulation and private initiative on the utilitarian model. That stage culminated in the Contagious Diseases Acts of the 1860s, but those regulations generated almost immediately a repeal coalition that employed a feminist and highly moralistic discourse that was suspicious of state-sponsored hygiene. The dominance of those voluntaristic purity crusaders lasted until the decade before World War I and scored many successes in criminal law. But the fears of national decline prompted by the Boer War encouraged a return to a medical and state-interventionist discourse that laid the foundations of the modern welfare state.

For all the nuance and detail in this book, the broad outlines of the changes in sexual discourses remain well within view. If the book has a flaw, it is its rather tenuous account of the scientific principles that undergirded both medical and moral discourses, particularly those of evolutionary biology and inheritance theory, which the author treats in an overly simplistic manner. This weakness is more than compensated for, however, by an impressive knowledge of public administration, popular culture, and the purity crusaders who sought no less than to put their moral stamp on the sexual behavior of a whole population.

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W. D. RUBINSTEIN. *Elites and the Wealthy in Modern British History: Essays in Social and Economic History*. New York: St. Martin's or Harvester, Sussex. 1987. Pp. x, 383. \$35.00.

That industrialization is associated with the creation of wealth and ultimately with changes in the distribution of income is widely accepted by economists and historians alike. Yet until comparatively recently, there was little in the historiography of modern Britain that examined the process in depth. Moreover, the process's consequences for the structure of society and its operation were neglected to a large extent by British social historians. Nearly twenty years ago, however, Harold Perkin's seminal work *The Origins of Modern English Society, 1780-1880* (1969) opened the debate on the relationship between society and the Industrial Revolution in England. There was consequently a burgeoning of interest in elites in British society, especially in the behavior of the emergent business elite. Of the research on the wealthy to emerge in the last two decades, W. D. Rubinstein's stands out as adding significantly to our understanding not only of the way income was distributed but also of the way in which the very wealthy behaved. Of value not only to social historians, Rubinstein's work straddles the boundaries of sociology and economics, social and economic history, while drawing conclusions of interest to the business historian.

Rubinstein's initial concern was to trace, using probate records, the very wealthy in British society who died between 1809 and 1939. That primarily statistical exercise led him to ask a number of interesting questions relating to the distribution, creation, and disposal of wealth in Britain during and after the initial period of industrialization. Those questions include a consideration of the main groups holding wealth, in terms of occupation and social standing; the impact of new wealth; and the geographical distribution of wealth. He is, moreover, concerned with the way in which the newly wealthy behaved, questioning the commonly held assumption that the majority bought appreciable amounts of land. The answers Rubinstein provides, supported as they are by painstaking research, are challenging, for they cast doubt on the idea that substantial new wealth was created by industrialists in the northwest of England. Instead, he suggests that most of the newly created wealth emanated from the commercial and banking groups in London.

Originally published in the *Economic History Review* in 1977 and expanded in *Men of Property: The Very Wealthy in Britain since the Industrial Revolution* in 1981, Rubinstein's ideas are now familiar to social and economic historians. It is that fact, rather than the quality of research, that is the main disappointment in this volume of essays. Of the eleven essays, six had already been published between 1977 and 1986 in the pages of the *Economic History Review*, *Past and Present*, and *Business History*. Of the rest, only one, on the geographic distribution of middle-class income, seems central to

the author's general thesis. Even that essay merely presents the statistical evidence lying behind conclusions he has already reached concerning wealth variations between London and the provinces. Nevertheless, his study of the evolution of the British Honours system and the emergence of a meritocracy is an important addition to the literature on the behavior of elites. The final three essays in the collection, all new, seem misplaced, not to say an indulgence. Dealing with the end of the "Old Corruption," the spirit of London as a city, and British radicalism, they sit uneasily in a volume concerned primarily with wealth, and they lack cohesion as a separate section.

This volume of essays, therefore, adds little to the debate on elites and the wealthy, for it is largely a compilation of published work. Given Rubinstein's reputation and his major contribution in the field, that is a great pity. It is unfortunate, also, that the inflated price of this book will reduce its attractions for those scholars wanting a collected volume of Rubinstein's essays.

MARY B. ROSE

University of Lancaster

MARGARET E. BRYANT. *The London Experience of Secondary Education*. Atlantic Highlands, N.J.: Athlone. 1986. Pp. xiii, 553. \$39.95.

Margaret Bryant's book should be treated as a treasure-trove, to be quarried from and foraged in by historians studying a variety of periods. As Bryant herself freely admits, secondary education is a time-bound concept; the term emerged only in the mid-nineteenth century. Different generations approach the socialization of their young in different ways. But at least from the Conquest on, some Londoners were concerned to provide access for some of their young to skills and knowledge beyond the most elementary modes of reading and writing.

Bryant has carefully and thoroughly chronicled such provision, taking her account as far as the end of the nineteenth century. Her material is of particular interest for historians of periods from the mid-eighteenth century on, when the taste for formal schooling seems to have expanded rapidly but when many of the enterprises that developed to gratify it were relatively ephemeral. Bryant has brought together information from a great variety of sources, many of them difficult to access, enabling us to form a much sharper view of the extent and diversity of this demand, at least in the capital city.

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LILIAN LEWIS SHIMAN. *Crusade against Drink in Victorian England*. New York: St. Martin's. 1988. Pp. x, 309. \$27.50.

The anti-drink movement in Victorian Britain has attracted considerable attention from historians in contrast to the drink trade itself (with its kaleidoscope of public houses, beer shops, gin palaces as well as brewers and distillers), which has been largely ignored. Lilian Lewis Shiman covers a good part of the same ground traversed by Brian Harrison in his *Drink and the Victorians* (1971). Starting in England about 1830 temperance was initially a middle-class movement whose members opposed spirits alone and urged moderation rather than total abstinence. In the late 1830s the movement was taken over by working-class activists committed to teetotalism and individual improvement. In the 1850s there was a further shift of direction as the United Kingdom Alliance (established in 1853) lobbied to curb the sale of drink by a series of prohibitionist bills in Parliament. For Harrison, the reformist Licensing Act of 1872 marked the climax of this story. But Shiman shows that the last decade of the century saw a renewed concern with personal abstinence and moral persuasion, together with a growth of large-scale support in the churches and the middle and lower classes that was manifested in the Church of England Temperance Society, the Blue Ribbon Army, the Rechabites, the Good Templars, and a motley of other organizations. A network of temperance communities and activities developed that was separate from the conventional social world lubricated by alcohol.

The surge of support for temperance in the 1870s and 1880s, linked to growing public concern over the problems of social dislocation and deprivation caused by industrial urbanization (temperance was never a rural movement in Britain), led to renewed political pressure. The Liberal party in 1891 included local prohibition in its Newcastle Programme, but, with the failure of the Liberals in the 1895 elections, the temperance movement went into decline. Unlike Harrison, who emphasized the interaction of temperance with national party politics, Shiman is principally concerned with the internal dynamic of the movement. She is particularly good at showing the difficulties of maintaining cohesion and harmony within a single-issue group. Bitter regional, class, party, religious, tactical, and personality conflicts repeatedly threatened the movement, causing a roller-coaster effect.

There are some good vignettes on the work of local societies, but one would have liked more on the social background of members (it is too simplistic to refer to most of them as "working-class"). In fact, the book is weakest on the social dimension—on the relationship of the movement to the changing economic and social conditions of late Victorian Britain. Too little is said about improving lower-class living standards and the proliferation of new types of leisure activities (which included some of the temperance groups) that steadily eroded the dominance of the public house in popular society. Again insufficient stress is put on legislation: the Licensing Act of 1872 that contributed to the steady decline of disorderly beer shops, along with other laws in 1882, 1902, and 1910. Those social and legal devel-

opments steadily contained the problem of alcohol consumption—per capita drinking reached a plateau in the last part of the century and thereafter fell sharply. Those factors may well have been crucial reasons for temperance's decline. Shiman's book has important points and material, but future work on Victorian temperance will need to show a clearer understanding of the changing pattern of the British drink trade.

PETER CLARK
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E. W. EDWARDS. *British Diplomacy and Finance in China, 1895–1914*. New York: Clarendon Press of Oxford University Press. 1987. Pp. 212. \$48.00.

Following China's defeat in the Sino-Japanese War of 1894–95, the imperialist powers focused attention on the weakened Asian giant, carving out "spheres of influence" and competing for concessionary privileges. As China was opened for mining and railway development and the Qing government looked to financial markets in the West to float loans to pay indemnities, reform its finances, and reorganize its administration, diplomacy and finance became inextricably linked.

E. W. Edwards charts the evolution of British policy in this changing environment. Since loans to the Chinese government brought favor and influence in Beijing, the British government became increasingly involved in sponsoring, guaranteeing, negotiating, and approving the terms of loans to China. The Hongkong and Shanghai Bank was the prime beneficiary of this new policy, receiving exclusive support quite at odds with earlier policies of official impartiality toward competing British firms. In exchange, the bank was compelled on occasion to allow political considerations to outweigh its own purely financial concerns.

A second set of changes began after the Russo-Japanese War of 1904–05 and brought a shift from international rivalry to international cooperation in financial affairs. Cooperation eventually bore fruit in 1911, when a consortium of British, German, French, and American interests provided a major loan to build the railway trunk lines through central China. This stage reached its climax in 1913 with a reorganization loan to the new Republic of China, a loan in which Russia and Japan joined the original consortium and the United States (under the Wilson administration) left. But this event marked both the peak and the end of cooperation, as the consortium broke up soon thereafter, unable to contain the competition of rival banks for investment in China.

The story of British diplomacy is told ably enough by Edwards, but one is a bit taken aback by this "tunnel history" in which the broader context of events is entirely blacked out. Anyone interested in learning something about China from this book will be either sorely disappointed or seriously misled. For example, railway loans were a key focus of financial diplomacy in China, and the consortium loan of May 1911 was the

most important of these. To gain that loan, the Chinese government had first to nationalize the provincial trunk lines. Edwards provides one comment only on this action, a citation in which the British minister in Beijing, Sir John Jordan, praised the Chinese government's decree as the "most statesmanlike pronouncement that the Chinese government has made on any question of policy in recent years" (p. 148). Edwards concludes his chapter on this success story by citing Jordan again: "'Railway matters are progressing splendidly . . .,' he wrote on 24 June 1911. Jordan's mood was soon to be changed. By mid-October much of China was in revolution" (p. 158). There is not a word to inform the reader that the revolution that broke out in October was a rather direct product of nationalistic agitation against foreign loans, one result of the government's "statesmanlike" railway nationalization.

Similarly, the reader is given little help in deciding whether the breakup of the consortium after 1913 had anything to do with the rising international tensions that led to World War I. Again the wider context is utterly missing. And the reason is fairly clear if one simply looks at the footnotes. In the final chapter, "The Revival of Competition," which covers 1913–1914, the footnotes include one earlier article by Edwards, ten other secondary works, fifty-six documents from the British Foreign Office, and three other primary sources. Especially when the subject is international competition and cooperation, we simply cannot endorse history written so exclusively from one side.

This book is pure diplomatic history in the old style. Such books have their merits. But in this case, the flaws are rather more visible.

JOSEPH W. ESHERICK
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LISA TICKNER. *The Spectacle of Women: Imagery of the Suffrage Campaign, 1907–14*. Chicago: University of Chicago Press. 1988. Pp. xii, 334. \$37.50.

A whole spate of books on suffrage and feminism in Britain has appeared in recent years, vastly expanding our understanding of the Victorian and Edwardian eras. Lisa Tickner's book, an analysis of the imagery of the suffrage campaign between 1907 and 1914, will stand as one of the most important contributions to an impressive body of work.

The book is stunning. Despite rapidly escalating and prohibitive publishing costs, the University of Chicago Press did not stint on production. The color plates beautifully reproduce suffrage posters on a scale that permits the reader to appreciate the impact they might have had on viewers. More remarkable even than the lavish illustrations is the text that they accompany. Using analytic tools drawn from semiotics, anthropology, feminist and critical theory, and psychoanalysis, Tickner, an art historian, provides us with new means of entry into the life and culture of Edwardian politics. Scholars will find this an exciting and rewarding work.

Two organizations—the Artists' Suffrage League and the Suffrage Atelier—produced much of the visual propaganda for the suffrage campaign between 1907 and 1914. "Recognising the role of popular imagery in the maintenance and reproduction of anti-feminism," Tickner asserts, "they threw themselves into . . . 'agitation by symbol' . . . , and by its means contested the representation of women, and their associations with all those attributes . . . that were used to argue their exclusion from the franchise and from public life" (p. 16). Feminists countered the misogynist and anti-feminist images of women put forth by commercial publishers and exploited by antisuffragists by producing their own representations—postcards, posters, photographs, and the like. They also produced themselves by taking part in mass meetings and marches designed to show the strength of their movement and, not incidentally, to display the women who supported it. Taking to the streets involved the risk that the public would associate feminists with the street, that is, with prostitution, thus jeopardizing rather than advancing the cause, and many women found it difficult at first to participate in public agitation. But "in so literally embodying their demand they were able to demonstrate not only the scale of its support but two related propositions: that all sorts and conditions of women wanted the vote, and that women who wanted the vote were not as they were popularly conceived to be in the public mind or caricatured in the illustrated press" (p. 59).

Because the British public believed that femininity was "written on the body" (p. 151), visual representations of women served more effectively to challenge conventional norms than written descriptions possibly could. Suffrage imagery and suffrage argument were inextricably intertwined; both sought to demythologize the dominant views of femininity and refused the distinctions between public and private spheres. "Suffragists and their artists," Tickner argues persuasively, "were inevitably concerned not just with the position of women in the economic and political spheres, but also with a struggle for meaning in all those representational—that is, ideological—practices where definitions of femininity were produced and contested" (p. 161).

The strengths of Tickner's sophisticated, yet thoroughly accessible, analysis of the suffrage movement are many. The author's prodigious research, careful use of theory, and comprehensive treatment of the period have produced a work of great significance.

SUSAN KINGSLEY KENT
University of Florida

GERRY R. RUBIN. *War, Law, and Labour: The Munitions Acts, State Regulation, and the Unions, 1915–1921*. New York: Clarendon Press of Oxford University Press. 1987. Pp. 275. \$55.00.

Early on in World War I, David Lloyd George declared Britain to be engaged in a "war of munitions," a war in

which the engineers of Britain were pitted against those of Germany. Such rhetoric was part of his endeavors to increase output from an industry suffering shortages of skilled men, partly because of the increased demand for skilled labor and partly because of the loss of men to the army during unrestricted voluntary recruitment. To achieve greater output, Lloyd George and the government needed the cooperation of the trade unions. Lloyd George achieved a voluntary agreement at the Treasury in March 1915. That agreement was given legislative force by Parliament as the Munitions of War Act of July 1915. One of its central elements was compulsory arbitration to avoid strikes and lockouts. Another aspect of the act was the setting up of munitions tribunals as special courts to hear cases of workers and employers infringing the act (and, after the end of the war, adjudicating on problems arising under the Restoration of Pre-War Practices Act of 1919).

Gerry R. Rubin's book is a detailed study of the workings of those tribunals, drawing mostly on the experiences of the Glasgow tribunal. It is an important contribution to the study of industrial relations in Britain during World War I. Rubin has mined a rich but narrow seam. Given that he has published seven articles in legal and historical journals as well as this monograph, he has worked that seam almost to exhaustion. The book provides a careful analysis of the response of the tribunals to the various categories of offense brought before them. Rubin provides a clear and usually convincing discussion of the importance of the tribunals' roles, setting his discussion within the political and economic context of the war. At times, however, his account becomes something of a study of the evolving case law of the tribunals. As he is a lecturer in law, that focus is not surprising.

Rubin provides a good discussion of corporatism and argues, contrary to recent revisionist writers, that "it is possible to advance a corporatist analysis of wartime labour developments without concentrating exclusively on the engineering industry, without identifying dilution as the central labour problem of the war, and without adhering to the existence of an unambiguous schism between the rank and file and trade union officialdom" (p. 260). He also shows that, when two of the Glasgow chairmen proved to be too independent in their judgments, the Ministry of Munitions had them replaced.

One of the merits of the book is its depiction of local trade union officials in action, arguing on behalf of their members before the tribunals and leading opposition to some aspects of wartime control of industry. It is a good antidote to the unrestrained attacks on union officials by William Gallacher (later a Communist party M.P.) in his memoirs. Rubin's account is also very interesting in showing how trade unionists could use the tribunals to criticize management and as a lever to gain concessions, what Rubin calls "collective bargaining by litigation" (p. 140).

My main disagreement with Rubin is over dilution of

labor (the use of semiskilled or unskilled labor, including women, on part of the work usually done by skilled labor). Rubin suggests that "it was not a practical problem of substance on Clydeside" (p. 241). He is right to see it as a problem not easy to bring before a court. But politically it was very much a major issue. Its resolution required the cooperation of Arthur Henderson and the leaders of the craft unions rather than actions at munitions tribunals.

Overall this is a good monograph dealing with an interesting and important aspect of the government's intervention in industrial relations during World War I. As such it is likely to remain the main work on the munitions tribunals for a long time.

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BERNARD WAITES. *A Class Society at War: England, 1914-1918*. New York: Berg; distributed by St. Martin's, New York. 1987. Pp. 303. \$37.50.

The aim of this study is to attempt to apply social theory to an understanding of the effects of World War I on English society. It would be pertinent to emphasize, in the first instance, that the author means England, and other parts of the United Kingdom have been deliberately excluded from the scope of this book. Although the intellectual justification for this focus is not substantiated—and its wisdom may be doubted by many readers—this study does succeed in bringing fresh analytical clarity to a subject that has been much dogged by impressionistic generalizations, and it introduces new empirical material on the "servile" and black-coated workers, a relatively neglected group.

Bernard Waites takes Arthur Marwick's work on the language and imagery of class as his point of departure, and what follows is a wide-ranging survey of the changes in wealth, income, work experiences, and more general social attitudes resulting from the Great War. The author's grasp of the secondary literature is impressive, and students of this period will welcome the lucidity as well as the comparative brevity of the presentation. Many will find uncontroversial the principal underlying argument that the effects of the conflict led to greater economic and cultural homogeneity not only among the working class but also among the middle and upper classes, but readers will benefit from the rigor with which Waites argues his case, and his chapter on the rapidly expanding salaried strata in many ways breaks new ground. In this respect, the author convincingly shows that neither an aversion to collective organization nor an antisocialist political outlook was a necessary feature of the consciousness of the black-coated worker in England during this period.

Despite Waites's explicit discussion of social theory and the frequency with which "structure" appears, I remain not entirely convinced about the integration of "structure" with "history" nor wholly persuaded by the internal consistency of its presentation. Waites ac-

knowledges in the preface that his approach is weighted toward "consciousness" rather than "structure." But what are the connections between the two? Much work recently in the social sciences has focused on the crucial role of agency in determining the outcomes of the interplay between institutions, consciousness, and social action. The author's analysis would have benefited from a consideration of this major area of theoretical discussion. Although historians have indeed been prone to use concepts in an unanalyzed and uncritical fashion, it does behoove them to get it right when they do embark on such a task. Waites's attempt to write "total history" is particularly found wanting in his penultimate chapter, "Secondary Education and Social Class," where scarcely five pages of text and tables are expected to suffice for an adequate survey of this important area and where there is almost no context provided relating this subject to the material dealt with in preceding chapters. Readers will also be disappointed by the book's neglect of the Great War's impact on gender relations, which has yet to receive the thorough investigation its significance deserves. Yet, as telling as these criticisms may be, this book makes a useful contribution to an understanding of the effects of the First World War on one of the principal combatants, and it sets the agenda for future research in this area.

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STEPHEN G. JONES. *The British Labour Movement and Film, 1918–1939*. (Cinema and Society.) New York: Routledge and Kegan Paul. 1987. Pp. ix, 248. \$69.50.

Stephen G. Jones has provided us with a model study in English cultural organization: at once comprehensive and compact, scholarly and readable, scrupulous and adventurous. Any hopes (such as the author himself seems tentatively to entertain at times) that the book will provide extensive new insights into popular culture are soon blighted, however.

With characteristic honesty, Jones concedes (p. 52) "the paucity of evidence" in dealing with perceptions even of the active labor movement—that "broad chapel consisting of political, industrial and cultural organisations," as he defines it elsewhere (p. 37). Much more elusive remains the question of "the way films were actually received and experienced by working-class cinema audiences" (p. 79). Recording genuine, spontaneous, and unmediated feelings may prove impossible for the most technologically sophisticated documenters of modern-day culture. How, then, can any historian begin to recapture the gut reactions of half a century ago? Evidence of more considered reactions is scanty enough, especially on the part of a class that had barely enough leisure to see films, let alone reflect on them in letters to the press or personal diaries. In the interwar years there was no equivalent even of those systematic

nosy parkers from the upper classes in which the Victorian age abounded—no Mayhews or Booths. And the Mass Observation movement was born only in 1936.

There are extant, however, some limited and random records kept by labor organizations themselves during the 1920s and 1930s, in the shape of subscription lists to film societies or attendance figures and reviews in publications such as the *Daily Worker*. It is from such scattered fragments that Jones has assembled his picture. It brilliantly evokes the vitality and enthusiasm of those societies, without blinking the ideological rifts that divided (and perhaps served to debilitate) them. In broader contexts—in his chapter on the various film workers' unions, for example, or when discoursing on the labor movement in general—the author can oversimplify these rifts: the basic division he discerns between "Marxist Socialists" and "Labour Socialists" ignores those activists who were not really socialists at all in any precise sense and were in the large majority, to deduce from Ross McKibbin's *Evolution of the Labour Party* (1974), a book oddly missing from Jones's bulging and catholic bibliography.

A greater majority, yet more debilitating to the growth of a distinctive labor film movement, were those audiences who (judging from sheer statistics, at least) continued to be more beguiled by the commercial picture palace than by the film society. Again, Jones does not blink this fact. Nor is he in any way condescending about the apparent preferences involved. But in the absence of nonstatistical evidence—the voices and impressions of the audiences themselves—he is at a loss to provide, or even attempt, an authoritative explanation of those preferences. To the history of cultural attitudes, what he can offer are new insights into tastes of an enthusiastic, but persistently marginal, kind.

Jones's book, however, is much more than a history of cultural attitudes and tastes, narrowly defined. He includes a chapter on the debate over the exhibition of films on Sunday that raises all kinds of religious and ethical issues that have still not entirely subsided today. Another chapter, "Labour, Film and the State," considers the political and bureaucratic dimensions of the medium and its potential for propaganda of all complexions. His chapter on the trade unions in the cinema industry shows how the subject of film belongs as much to the history of work as to the history of leisure. In its modest way, this book aspires to—and largely succeeds in achieving—the condition of "total history."

IAN BRITAIN
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STUART BALL. *Baldwin and the Conservative Party: The Crisis of 1929–1931*. New Haven: Yale University Press. 1988. Pp. xix, 266. \$35.00.

This retelling and reinterpretation of a familiar story is based on impressive research not only in the papers of

Conservative leaders, other M.P.'s, peers, and the national party archive but also in well over a hundred regional and constituency records. Though not without shortcomings, it is a rare and admirable attempt to fuse high and low politics, to relate the aims and motives of party leaders and the local constituency elites, in order to explain what happened in the Conservative party during two crucial years for British party politics and to do so from a wider perspective than Westminster and the capital.

The unexpected defeat of the Conservative government in the general election of 1929, when it sank to 260 seats from the 415 it had won in 1924, and the formation of a second minority Labour government sustained by the Liberals caused a crisis of confidence, leadership, and direction in the Conservative party. Stanley Baldwin was not an impressive leader of the opposition and remained sunk in apparent apathy except when his position was in real danger. But Stuart Ball maintains that the real crisis was produced not by the efforts of the press lords to assert their power over the party, not by Lord Beaverbrook's free trade crusade, or by Winston Churchill's attempt to seize the party leadership by rallying the opponents of Indian self-government but by pressure from the constituencies for a voice in party policy and accountability from the leader. As the economic situation worsened, the local leaders were convinced that success at the polls would be ensured by a program of antisocialism and "economy," a term that decently covered tariffs, tax cutting, and reduction of social benefits. Their champion was the dynamic Neville Chamberlain, who replaced the ineffective and abrasive J. C. C. Davidson as party chairman in June 1930 and whose views, Ball argues, were closer to the constituency leaders' defense of wealth than to his father's social imperialism.

Chamberlain and his hard-faced associates had no interest in a coalition before the financial crisis of August 1931, believing that they were certain to win a majority at the next election, but they accepted George V's strong urging of a national government under Ramsay MacDonald when they realized that they would be able to carry out their antisocialist program without charges of class legislation. The national government that resulted also had the gratifying effect of dealing a severe blow to the Labour party, destroying the Liberals, and reducing the three-party system of the 1920s to the simple choice between one party in favor of socialism and one against.

In Ball's view there is a fundamental unity to the twentieth-century Conservative party, exemplified in the principles that came to the fore in the crisis of 1929-31, from which the policies of R. A. Butler and Harold Macmillan were an aberration. Ball's is a stimulating viewpoint, but, to be convincing, he needs to elaborate his argument within a wider reconsideration of Conservative social and economic policies. Although there is undoubtedly much to what he says, Ball has perhaps been too impressed by the views of constituency figures, who are generally more extreme than the

leaders. No one was better at outmaneuvering and even deceiving his ardent followers in order to achieve power than Baldwin. He may have been puzzled about how to keep the party on a moderate course acceptable to the electorate after defeat in 1929 when he was surrounded by right-wing M.P.'s from safe constituencies, but, when the opportunity came to control events while freeing himself from the extremists, he did not hesitate to seize and hang on to it.

NEVILLE THOMPSON

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JOHN W. YOUNG, EDITOR. *The Foreign Policy of Churchill's Peacetime Administration, 1951-1955*. Leicester: Leicester University Press. 1988. Pp. x, 273. £25.00.

This volume is a collection of essays dealing with major issues of British foreign policy in the period 1951-55. It was Winston Churchill's last administration, from which Anthony Eden emerged, however briefly, as prime minister. It was a Conservative administration, of course, and its principals were already familiar actors in British foreign policy from the years of the Second World War. They march across these pages in higher and often more important roles than the ones in which they served during the war and remind us of how young (relatively) they were in the years 1939-45. Harold Macmillan, Anthony Nutting, and Evelyn Schucking are only a few of the names well known to students of British policy during the war years.

Editor John W. Young has written a third of these essays plus the introduction. Issues covered include the Anglo-American "special relationship" (Peter Boyle), cold war and détente (Young), Germany and European defense (Young), European unity and the United Kingdom (Young), Suez (Ritchie Ovendale), Iran (Brian Holden Reid), South Africa (G. R. Berridge and J. E. Spence), the Korean War settlement (Peter Lowe), and the Indochina war settlement (Geoffrey Warner). It is a particularly excellent collection, well conceived, well written, and instructive. The central theme of evolving Anglo-American relations as affected by these issues binds the essays together into a coherent whole, and each almost constitutes a chapter in a monograph. They analyze the statesmanship of Anthony Eden and the political values of Winston Churchill and shed light on the extent to which Whitehall came to view American statecraft as both naive and trigger-happy.

Young's introduction assesses policy making in the period, describing the impact of the "retreat from power," economic and political, and Churchill's determination to prevent the retreat from actually reducing Britain's great power image. One result was that Whitehall turned into a collection of "warring baronies" (p. 22) over external policy as each department fought for its share of a shrinking budget. Perhaps this did not matter so long as the Anglo-American relationship was special, which it was because of Churchill's personal ties with Washington. London found a number of Ameri-

can developments of the period alarming, however, especially the outpouring of rabid anticommunism during the McCarthy period, which complicated East-West relations and expanded the cold war.

But, in looking askance at aspects of American policy formulation and political thinking, Whitehall could not erase the negative aspects of its own perception of the world. Interference in Iranian internal politics (the downfall of Mohammad Mosaddeq) was one example, and, although the American Central Intelligence Agency may have played a major role in Iran, it did so with the connivance of the British government. Meanwhile, the Suez base agreement did not resolve the difficulties that soon led to the invasion of 1956, Eden's great folly. The authors in this volume spare no one, nor do they fail to point out competence and reason when they occurred in the policy-making process.

The contributors to this volume made use of archival sources only recently available to scholars in the Public Records Office in London, and these essays demonstrate that the present generation of British diplomatic historians is as adept as its mentors of past decades. The essays are likely destined to point the way to further and more extensive research on these and related issues. They are very welcome, and very timely, in the bargain.

ROBERT COLE
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DUNCAN WILSON. *Gilbert Murray, OM: 1866–1957*. New York: Clarendon Press of Oxford University Press. 1987. Pp. xii, 474. \$54.00.

Gilbert Murray's public career stretched from the 1880s, when he was elected to the professorship of Greek at Glasgow University, to the 1950s, when he championed Anthony Eden in the Suez crisis. The best-known classicist of his generation, Murray won renown in politics and the arts as well. His friend and fellow dramatist George Bernard Shaw based characters in *Major Barbara* on Murray and his wife, Mary. Another celebrated friend, H. A. L. Fisher, lamented the "public scandal" that Europe's foremost Greek scholar was spending so much of his time heading the League of Nations Union.

A life so long and varied challenges the biographer. Duncan Wilson skillfully threads his way through the scholarly and political byways of Murray's career to produce this sympathetic study. Wilson, a diplomat, classicist, and biographer, used sources not available to earlier writers, and he was assisted by the Murray family. Wilson died in 1983 before this book was quite finished, and his widow, Elizabeth Wilson, completed it.

If Wilson had lived, he might have dealt with some issues raised by Francis West's brief study of Murray, which appeared in 1984. West had much to say about Murray's supposed reconversion to Catholicism on his deathbed, an event of some significance given Murray's

well-known "rationalist" credo. He had, after all, been the model for the "Good Pagan" in his daughter Rosalind Toynbee's manifesto, *The Good Pagan's Failure*. Wilson dismisses the episode. He does, however, put Murray's rationalism into an interesting perspective by discussing his many experiments with spirituality and his claim to have psychic powers.

Even as Wilson explores the many paths of Murray's long career, he never loses sight of Murray's main objectives in life, understanding ancient Greece and achieving peace between nations. In both of those areas, Murray was an easy target for criticism. His translations were the subject of a bitter attack by T. S. Eliot, and few scholars in this age of specialization would claim that he had much influence by the closing years of his life. In his peace work, Murray was also vulnerable, his cause of the League of Nations easily dismissed.

Wilson patiently examines the evidence and agrees that Murray's interpretations of classical authors may have been cast in terms of his own age. Although granting the fairness of some criticisms, he puts Murray's scholarship into perspective, reminding us that his verse translations enjoyed great success on the stage as well as in books and that contemporary taste is no final arbiter. He is also able to show the incredible dedication and energy that Murray brought to his work at the League of Nations and the International Institute of Intellectual Cooperation.

If Murray's causes sound dull today, his life was, as this biography shows, exceedingly lively. From his unusual courtship of an earl's daughter and the noisy protests when he won his chair at Glasgow at the age of twenty-three until his death in 1957, Murray was a fascinating figure. A man of principle, indeed of a great many principles, he could still bend his principles to suit the occasion.

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A. G. JAMIESON, EDITOR. *A People of the Sea: The Maritime History of the Channel Islands*. New York: Methuen. 1986. Pp. xxxvi, 528. \$89.95.

In an aptly selected introductory quotation from Victor Hugo's *Les Travaillleurs de la Mer*, we are told that the population of Guernsey is composed of men who have passed their lives going around in fields and of men who have passed their lives going around the world. The two kinds of laborers are the tillers of the land and the toilers of the sea. This large, attractive volume attempts to provide for the reader the historical foundation for Hugo's perceptive comment.

A. G. Jamieson successfully introduces the book and contributes ten of the seventeen essays that make up this volume. The book is divided into two parts, "The Centuries of Emergence," to 1689, and "The Maritime Heyday," which extends to the present. Contributions

by a number of specialists lend veracity and range to the volume.

Barry Cunliffe assays the archaeological record to A.D. 1000, and Wendy Stevenson continues the story to 1500. Channel Island shipping is surveyed for the period 1300 to 1500 by Wendy Childs and for the period 1500 to 1689 by John C. Appleby. John Bromley's essay "Privateering and War, 1689–1713" follows, complemented by William Meyer's chapter "Privateering, 1793–1815." Rosemary Ommer adds a chapter on the cod trade in the New World. Jamieson's contributions cover topics on ships, shipbuilding, trade, harbors, lighthouses, and fisheries after 1600. The essays are freestanding pieces but do blend together sufficiently to form a narrative account of the Channel Islands that is based on a foundation of sound scholarship.

The Channel Islands are composed of Jersey (forty-five square miles), Guernsey (twenty-five square miles), Alderney (three square miles), Sark (two square miles), and smaller islands, reefs, and rocks. Because they lie as close as ten miles to the French coast, the political advantages of English control of the islands are evident. The islands' fortunes rose and fell, reaching their economic peak in the eighteenth and nineteenth centuries.

Space does not permit analysis of the individual chapters, but the reader can be assured that the contributions form a coherent and well-documented work. Much of the book is founded on primary sources credited in the footnotes, and a thorough bibliography is provided. For the medieval period, the pioneering work of John LePatourel is properly acknowledged and expanded upon. The team that has produced this sympathetic study of the Channel Islanders might be criticized for failing to develop a historical narrative rather than selected specialized chapters obviously suited to the contributors' interests, but the work as a whole stands as an important contribution to maritime studies.

TIMOTHY J. RUNYAN
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ALAN ARMSTRONG. *Farmworkers in England and Wales: A Social and Economic History, 1770–1980*. Ames: Iowa State University Press. 1988. Pp. 302.

In the tradition of Jonathan Chambers and George Mingay, Alan Armstrong has produced a masterful up-to-date analysis of the economic history of modern English agriculture. Generation by generation, he carefully reconstructs the farm workers' wages and standards of living, making allowances for unemployment, regional variations, and perquisites, especially housing. He also suggests occasional international comparisons. He finds that the farm workers' fortunes deteriorated in the later eighteenth century, were buoyed slightly by the wartime boom, sank to their nadir in the turbulent 1830s and hungry 1840s, and

then began to improve in the next two generations. Allowing for Edwardian stagnation, wartime fillip, and interwar decline, the twentieth century has seen a marked rise in farm workers' living standards, aided by considerable flight from the land and government policies of welfare programs and farm supports. Armstrong's conclusions create a valuable base line for the broader history of two centuries of popular standards of living.

He finds the farm workers' lot to have been determined by the fluctuating prosperity of "the agricultural interest"; by the demand for and supply of labor, especially as affected by demography, including domestic and foreign emigration; by government policies; and by a regional north-south contrast that only died out in the interwar period. But, in contrast to his crisp and lucid treatment of the enclosure controversy, Armstrong does not render a clear verdict on the vexed question of the economic effects of the Poor Law, ambiguous references to "ill-advised parish allowances" and a "partly real, partly artificial" winter surplus of labor notwithstanding (pp. 65, 69). Although he discusses farm mechanization very helpfully, he does not depict much of the farm workers' actual work. Hence, we are unprepared for his remark that today's workers prefer the "farm worker" of his title, which implies skilled occupation, to the older "agricultural labourer," which summons up the gaping "Hodge."

Armstrong does not match his economic history with the same rigorous analysis of the social history of the farm workers. They emerge from this study not as sentient subjects but as mute objects of processes beyond their control. The author makes little use of the autobiographies collected by John Burnett, David Vincent, and Paul Thompson. His sturdy demographic skeletons lack the flesh of wedding ceremonies, family affections, the feelings of liberation or dread experienced by emigrants to Pennsylvania, Botany Bay, or the trenches of Gallipoli. Surely folklore might have shed light on mentalities and customs. The quality and dynamics of village society are not reconstructed beyond scattered references to village studies. The question of social relations and mentality is reduced to a simple one of class, pro and con. Although Armstrong's answers to his narrowly framed questions are probably right—farm workers were late in developing a weak class consciousness and trade unions—more could be said. Social protest is treated as a simple reflex of economic distress, and the widespread Swing riots are regarded as an "episode" rather than a revealing eruption of attitudes and relationships. The Tolpuddle martyrs were doubtless "unlucky to fall foul of so fierce a reactionary as Frampton" (p. 78), but how should we explain the structures of power and social relations that promoted and permitted that victimization? Armstrong gestures toward the nuanced questions about deference raised by Howard Newby, James Obelkevich, and Robert Moore but does not reflect

fully on them, a pity because his learning and analytical powers would make his perspective most valuable.

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JANE MORGAN. *Conflict and Order: The Police and Labour Disputes in England and Wales, 1900–1939*. New York: Clarendon Press of Oxford University Press. 1987. Pp. viii, 305. \$59.00.

During the Victorian era, the British police acquired a reputation as a restrained, disciplined, civilizing group in a divided society, and the police enjoyed support or at least acceptance from most social groups. Historians writing during the last twenty years have looked more skeptically at the tradition of police civility, asking both whether it fits nineteenth-century reality and whether it will survive today's bureaucratic isolation and repressive police handling of labor disputes and racial conflicts in Britain.

Jane Morgan's book is a valuable addition to this reexamination. She finds the roots of modern centralization and lack of accountability in the labor conflicts of the early twentieth century. Recurring strikes through the mid-1920s led officials, who viewed the trade unions as a threat to social stability, to reshape the police into a more centralized and independent and less restrained institution. There are several actors in Morgan's story. The Home Office sought greater control over policing labor disputes. The army increasingly served, though not always enthusiastically, as an internal police force in cooperation with the Home Office. The local magistrates, who were often industrialists, and chief constables sought more power to deploy and reinforce the police to aid in repressing strikes. Finally, the local governing bodies or councils struggled to retain their control over the police. They were often more sympathetic to the strikers and concerned that policing not simply aid the industrialists in strike breaking. The local communities were able to resist centralization for several years despite the efforts of the Home Office to tie police administrators to the central government, but emergencies during and after World War I encouraged chief constables to look to Whitehall for direction. The coordination of military and police forces in the General Strike of 1926 confirmed that the police had become "national in spirit if not in form" (p. 277).

In addition to centralized direction, police forces themselves acquired increased power to control picketing and organized protest marches through various emergency regulations during periods of turmoil and through ordinary labor legislation and court decisions. Along with legal power came an increase in police arbitrariness and rougher tactics for which they were rarely called to account. The police became by 1939 not only more centralized but also more sympathetic to the industrialists in labor disputes. Despite these developments, Morgan reminds us that repression of strikes

and picketing was far less violent in Britain than it was in France or the United States and that the police retained considerable working-class acceptance of their ordinary duties of law enforcement and maintenance of order.

Lack of space prevents me from doing justice to the nuance and complexity of Morgan's argument. My only criticism is of the organizational structure, which treats centralization and the increase of police powers in two distinct sections of the book that separately cover the same years. A narrative in which both themes were developed together would have strengthened the argument by conveying their interrelationship. Overall, this is an excellent book, valuable to all interested in British society and its police because Morgan is the first to explore a period and problem of police development that has important consequences today.

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R. R. DAVIES. *The History of Wales*. Volume 2, *Conquest, Coexistence, and Change: Wales, 1063–1415*. New York: Clarendon Press of Oxford University Press or University of Wales Press, Cardiff. 1987. Pp. xv, 530. \$76.00.

Since its publication in 1911, Sir John Lloyd's *History of Wales from the Earliest Times to the Edwardian Conquest* has held the field as the only substantial scholarly account of the history of medieval Wales. Readers of R. R. Davies's previous book, *Lordship and Society in the March of Wales, 1282–1400*, and of his many articles know that any general study of medieval Wales from his pen will be of major importance. His new book, which forms part of a series on the history of Wales, fulfills every expectation. It is deeply researched and lucidly written, and, although Davies's study begins and ends at different dates, it is a worthy successor to Lloyd's pioneering work.

But this book is more than just an up-to-date replacement for a distinguished product of an earlier generation of scholarship. Davies gives full measure to the history of the native ruling dynasties of Wales and, in particular, to the attempts by the thirteenth-century princes of Gwynedd to create a state sufficiently coherent and well enough administered to survive outside attack. He then devotes a great deal of careful attention to the ways in which Wales was influenced and changed by the long-drawn-out period of conquest that began with the Anglo-Saxon victory over Gruffudd ap Llywelyn of Gwynedd in 1063, was pursued through the creation of the Norman lordships in the Welsh March in the late eleventh and twelfth centuries, and was brought to completion by the conquest of independent Wales by Edward I in 1282–83.

Davies treats postconquest Wales with as much insight and sympathy as he does the Wales of the earlier period, and the result is a clearly and convincingly drawn picture of the assimilation of Wales into the

newly imposed English governmental system, of the settlement of English colonists in the boroughs founded alongside the strategically placed castles in north Wales, and of the growing tensions within Welsh society, which could have produced a revolt on several occasions in the fourteenth century. That revolt finally occurred in 1400 under the leadership of Owain Glyndŵr, and, with its failure, the conquest of Wales can finally be said to have been completed by 1415.

Not the least of the strengths of Davies's book is that he also places medieval Wales firmly within the context of British and Irish history and of European history in general. For that reason alone, Davies's book deserves the attention of all specialists in medieval history. For those who are interested specifically in medieval Wales, this book is a work of the first rank.

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GERAINT H. JENKINS. *The History of Wales. Volume 4, The Foundation of Modern Wales, 1642–1780*. New York: Clarendon Press of Oxford University Press. 1987. Pp. ix, 490. \$78.00.

The history of the Welsh nation is a complex tapestry of intrigue, civil strife, religious turmoil, arranged marriages, and family feuds, woven with a landscape as backdrop described by Ned Ward in his book *A Trip to North Wales* (1701) as "the fag-end of Creation; the very rubbish of Noah's flood." Located at the northwestern edge of Europe, Wales responded slowly to events beyond its boundaries, and the population was controlled as much by the changing seasons and by tradition as by the will of Parliament.

This book is the fourth volume in a series of six that represents the first complete standard history of Wales. It covers 140 years in which Wales and the Welsh were dragged from a feudal, almost changeless era through a civil war that had almost no relevance to the Welsh nation, and the work concludes at a time when the iron and coal industries heralded unparalleled changes.

Geraint H. Jenkins concentrates on the social forces that shaped the events of the day. The contents of the book fall conveniently into three sections. The period from 1642 to 1660 was the era of the Civil War and Puritanism. Jenkins explains clearly why both of these causes failed to attract the interest of the Welsh. The outlawing of the Welsh language was a vain attempt by the Puritan schools to break the population of its traditional loyalties. Puritanism itself split into numerous subgroups, not only in Wales but also in northern England, Ulster, and Scotland. But, even here, Wales took an independent approach, favoring the Congregationalist and Baptist sects rather than the Presbyterianism that dominated elsewhere.

Religion, music, and literature formed the cornerstones of Welsh society throughout the period covered by this book. Chapters 3 to 6 examine these aspects and

concentrate particularly on the years 1660 to 1730 when the richness of Welsh society in terms of its cultural development reached its zenith. To outsiders, (notably to the English), little of this native quality of life could be discerned, for it took place in the chapel and in the homes of ordinary men and women and was conducted through the Welsh language.

The final section of the book concentrates on the social and economic progress of the fifty years leading to 1780. Improvements in agriculture, the gradual control of disease, and the emergence of industry all led to a rapid growth of the population and to a shift in its distribution. This period showed little decline in the cultural quality of life in Wales. If anything, interest in education and self-improvement increased. Literary societies flourished, and Wales and its people gradually developed the confidence to look outward, a sign of maturity certainly but also a trend that contained within it the means of diluting the uniqueness of the Welsh way of life.

Jenkins captures the moods of the time with great sympathy. Attention to detail is well balanced with anecdotal quotation written by commentators contemporary to the eras he examines. The text is alive and makes compelling reading for student, teacher, and general reader. For the serious student of history, a full bibliography and a list of manuscripts and their origins are provided.

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ALICE STROUP. *Royal Funding of the Parisian Académie Royale des Sciences during the 1690s*. (Transactions of the American Philosophical Society, number 77, part 4.) Philadelphia: The Society. 1987. Pp. 167. \$15.00.

This meticulously researched study of the financing of the Paris Académie Royale des Sciences during the protectorship (1691–99) of the French minister of finance Louis Phélypeaux de Pontchartrain is a separately published part of Alice Stroup's forthcoming larger history of the Académie in the seventeenth century, *A Company of Scientists*. Although best read together with that companion volume, this study deserves attention in its own right because Stroup has managed to show definitively that Jules Guiffrey's conjecture that the French crown ceased funding the Académie in the 1690s was mistaken. Guiffrey had made his conjecture when, in editing the *comptes des bâtimens*, which contain the detailed records of the Académie's expenditures, he found that those expenditures largely disappeared after 1690. That disappearance, however, reflected a change in fiscal record keeping, as the pensions for scientists during Pontchartrain's ministry were indiscriminately recorded together in one lump sum with pensions for the wholly distinct Académie Royale des Inscriptions in the *comptes du trésor royal*. Until Stroup untangled the confusion of fiscal records involved in the switch from the buildings

account to the general treasury account, historians possessed no exact analysis of what happened to royal investments in the Académie Royale des Sciences after the protectorships of Jean-Baptiste Colbert and François Michel Le Tellier, marquis de Louvois.

It is precisely because Stroup succeeds so well in reconstructing the amounts of pensions and other expenditures from the general treasury account that her work raises for the reader several intriguing questions. Pontchartrain was a cultural administrator who, like a seventeenth-century William Bennett, sought to improve the conduct of scientific research at the same time that he deliberately cut royal investment in the Académie in both relative and absolute terms. As Stroup notes, the budget of the much smaller Jardin Royal, a teaching institution founded to modernize medical training in Paris, increased while that of the Académie decreased under Pontchartrain's direction. How did such a finance minister conceive of his own aims? What role did he believe the state should play in the definition of science and of French culture as a whole? What was the relationship between the precarious fortunes of scientific inquiry during the reign of Louis XIV and the fortunes of other government-funded cultural enterprises? These are complex questions to be sure, but Stroup's study of funding for the Académie in the 1690s would be even more effective if it offered some suggestions concerning Pontchartrain's motivations and if it offered more interpretation of the differences in funding levels between the Académie and other institutions, such as the Jardin Royal and the abbey of Saint Germain des Prés.

Having done such a splendid job of giving us the financial facts in her comparisons of the Académie with the Jardin Royal and the abbey of Saint Germain des Prés, perhaps in a future work Stroup will speculate a bit more about the meaning of those facts. The present study limits itself to an excellent exposition of which scientific projects funded under Colbert or Louvois continued to be funded by Pontchartrain and what changes of emphasis in the Académie's activities Pontchartrain encouraged. Its value to historians of science and historians of France lies in its rigorous attention to detail.

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GERD VAN DEN HEUVEL. *Der Freiheitsbegriff der Französischen Revolution: Studien zur Revolutionsideologie*. (Schriftenreihe der Historischen Kommission bei der Bayerischen Akademie der Wissenschaften, number 31.) Göttingen: Vandenhoeck und Ruprecht. 1988. Pp. 293. DM 92.

Gerd van den Heuvel is the author of a short study on the French peasantry in the eighteenth century and one of the editors of *Handbuch politisch-sozialer Grundbegriffe in Frankreich 1680–1820* (1985–), a collection of 150 studies of major French social and political con-

cepts and how the meaning of those concepts changed from the Age of Louis XIV to the Restoration. This study of the French concept of freedom (*liberté*) from the beginning of the eighteenth century to Napoleon Bonaparte's seizure of power is a more detailed application of the methods of *Begriffsgeschichte*, the specifically West German attempt to combine intellectual and social history.

Compared to the Germans, van den Heuvel finds, the mid-eighteenth-century French had abandoned the corporatist identification of liberties as the privileges of corporate groups within the framework of Old Regime society, but, he argues, the Lockean definition of liberty as an inherent right of individuals in opposition to the state found little acceptance in France. (He may well exaggerate the extent to which that concept of liberty had triumphed in eighteenth-century England and North America.) Jean Jacques Rousseau's argument that freedom meant participation in but also submission to the laws of an all-encompassing state was, van den Heuvel asserts, merely the clearest statement of an attitude that was in fact widely shared during the French Enlightenment. He thus sees little rupture between the revolutionary period and what had preceded it. The individual liberties stipulated in the Declaration of the Rights of Man in 1789 were explicitly made subject to legal limitations, and, even among the revolutionaries, unrestricted individual freedom continued to be stigmatized as "license." In any event, liberties as the tangible prerogatives of private citizens were quickly overshadowed by a quasi-religious veneration of liberty as an abstract symbol, counterposed to the discredited symbols of the French monarchy. The Jacobin period saw the cult of liberty developed to its greatest extent at the very moment when specific civil liberties were most drastically curtailed. Van den Heuvel concludes by arguing that the Thermidorian and Directory periods, often dismissed as a sterile interregnum between the heroic years of the early revolution and the equally dramatic reign of Napoleon, were in fact the crucial moments when the more "Anglo-Saxon" notion of liberty as inprescriptible individual right finally took hold in France.

In keeping with the dictate of *Begriffsgeschichte* to go beyond conventional high-culture sources, van den Heuvel makes creative use of evidence such as political slogans and revolutionary engravings. Even so, few of his conclusions are completely new. Readers who know Nannerl Keohane's discussion of Rousseau in *Philosophy and the State in France* (1980) or Lynn Hunt's analysis of revolutionary imagery in *Politics, Culture, and Class in the French Revolution* (1984), neither of which van den Heuvel cites, will find that those works anticipate some of his most important arguments. Nor does van den Heuvel really manage to penetrate the minds of the lower classes. He does not emulate Dale Van Kley's *The Damiens Affair and the Unravelling of the Old Regime* (1984) by using archival sources to explore popular political attitudes. The result is, nonetheless, a

solid study that deepens our understanding of the French notion of liberty in a critical period.

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L. E. SHINER. *The Secret Mirror: Literary Form and History in Tocqueville's Recollections*. Ithaca: Cornell University Press. 1988. Pp. xvii, 224.

In this book the philosopher L. E. Shiner has tapped two subjects of recent interest to historians: Alexis de Tocqueville and literary theory. François Furet's use of Tocqueville in 1978 to rethink the French revolution did not initiate so much as join a growing body of scholarship on the conservative Anglophile. Within the past twenty years, Seymour Drescher, Edward Gargan, James Schleifer, and André Jardin, among others in political science and sociology, have made notable contributions in the field. Furthermore, intellectual historians such as Hayden White, Dominick LaCapra, and John Toews have explored the implications of literary theory for historical practice. Structuralist, semiotic, reader-response, and deconstructionist perspectives on texts—the basis for most historical research—have altered the way historians read. The result is a greater sensitivity to theoretical problems and issues in history.

In this study Shiner applies literary theory to Tocqueville's *Recollections*, a major document on the year 1848 written by an eloquent eyewitness. Without much thought of literary context, authors of textbooks on the revolution often quote lines by Tocqueville on Louis-Philippe, Auguste Blanqui, Louis Blanc, and Odilon Barrot. But the text, Shiner argues, has a more complex design. In Shiner's well-crafted analysis, Tocqueville's work is more than a memoir, a mirror of events actually experienced and recorded for posterity. The *Recollections* is, rather, a confluence of constituent genres—the portrait, the tableau, and the aphorism—as well as a historical and personal account. What Shiner finds in the text itself is an underlying tension between its rhetorical features on the one hand and its narrative structure on the other. Opposing generic and plot elements thus create a discordance that suggests an implicit moral and political code. Tocqueville's recollection presents dramatic scenes whose meanings are derived from an opposition of specific virtues and vices. A moral polarity between greatness and greed, especially, underscores other polarities between social classes, political ideals, institutions, and literary forms. Accordingly, *Recollections* is "not a construction made up of an underlying skeleton and its stylistic clothing, so much as it is a texture produced by the interweaving of synchronic and diachronic devices" (p. 97).

A similar complexity emerges in the two major voices speaking in the text; recollection and commentary define Tocqueville's relation both to the reader and to the content of the text. The characteristic rhythm of voices in the *Recollections*, or its style, is determined by

the way that the author deploys various modes of discourse. The resulting texture of discursive viewpoints is the structuralist version of the authorial presence in the work. But it is also the basis for Shiner's illuminating comparison of Tocqueville's *Recollections* with Karl Marx's *Class Struggles in France* and *Eighteenth Brumaire of Louis Bonaparte* and again with Gustave Flaubert's *Sentimental Education*. The rhetorical strategies of these works underscore the differences between them and their references to events in 1848. Similarly, Shiner examines Tocqueville's other important writings to characterize and compare their distinctive structures. Shiner's book then concludes with a brief discussion of the autonomy of historical writing from the writer, the relativism in literary analysis of texts, and the usefulness of applying literary theory to historical work.

This book is significant for its clarity and insight into the nature of textual sources. In his forthright approach Shiner explores the complex linguistic context of an important historical document. With a minimum of jargon, the author also provides historians with issues to consider in similar texts. Only a few misspellings and minor factual errors mar this otherwise valuable consideration of both Tocqueville and theory, useful reading indeed for everyone active in these two fields.

JAMES SMITH ALLEN
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PIERRE BIRNBAUM. *Un Mythe politique: La "République juive"; De Léon Blum à Pierre Mendès-France*. (Nouvelles études historiques.) Paris: Fayard. 1988. Pp. 417. 140 fr.

Jean-Paul Sartre's classic essay *Réflexions sur la question juive*, published in 1946, had the dual purpose of uncovering the human sources of anti-Semitism and condemning its protagonists in France. Whereas Sartre was concerned with the persistence of anti-Semitism in liberated France, Pierre Birnbaum is intrigued by the constant social presence of anti-Semitism in French society from the close of the nineteenth century to the present. In contradistinction to Sartre's social-psychological assessment of the anti-Semite and his purely theoretical model, Birnbaum sees a direct link between the nature of the French state and the torrent of anti-Semitic excommunications so diligently assembled in this highly provocative and original book.

Following his extensive studies on the modern state, Birnbaum sees France as a "strong state," characterized by a formidable bureaucratic administration and an internal drive for a distinct identification of the individual with the state. Notwithstanding its secular and universal nature, the French republican ethic of statehood was imbued with hostility to any form of particularism and countered expressions of political pluralism. In Birnbaum's view, it is in such strong states, as opposed to those in decline or lacking a similar cohe-

siveness (for example, the United States and England), that political anti-Semitism tends to flourish. Yet identification with the republican state seemed to sit well with the Jews of France, who actively pursued public careers and twice reached the pinnacle of statehood. Herein lies the issue that is at the center of Birnbaum's inquiry: the correlation between the active integration of Jews into a strong state system and the emergence of political anti-Semitism with the examples of Léon Blum and Pierre Mendès-France serving as the main test cases.

Evaluating the social, political, and Jewish perspectives of these leaders, Birnbaum carefully distinguishes between their mutual concerns and unique orientations. Both appear to him deeply committed to the permanence of the state structure and its ethical possibilities, though they did not share the same emphases on how the state could be instrumental in dealing with different social concerns. Drawing on a wealth of new material, which certainly challenges the approach of Jean Lacouture, a biographer of both figures, Birnbaum convincingly shows how Blum and Mendès-France, to varying degrees, maintained an affirmative relationship with their Jewish backgrounds, even in the face of recurrent anti-Semitic charges (pp. 61–85, 230–36). Here, with good reason, Birnbaum also takes issue with the accepted image of native French Jewry as a community that relinquished all forms of identification on its road to full integration into French society. Blum and Mendès-France emerge from Birnbaum's treatment as individuals who asserted without equivocation the principles embedded in the republican state, rejecting the apparent contradiction between loyalty to the state and particularism. Their religious background was not lost on members of the French Jewish community, who in their diverse attitudes toward political Jewish leaders in France illuminated the community's heterogeneity. But at the center of the author's attention is the inability of French society to ignore Blum's and Mendès-France's particularistic associations.

Where did the tremendous outburst of anti-Semitism stem from? Birnbaum contends that, notwithstanding Edouard Drumont's fundamental contribution to French anti-Semitism, he signified the end of an epoch rather than a beginning. Drumont's lacerating attacks against Jewish economic domination (accentuated by other images of the Jews as cosmopolitan, sexually perverted, and nomadic) were part of a traditional harangue that failed to take into consideration a significant change in Jewish society—its involvement in the French public sphere. Birnbaum does not imply that this form of anti-Semitism withered away; he painstakingly shows its resilience within French society. But, unfortunately, the large, previously unmined corpus of anti-Semitic references he has accumulated to show Drumont's continuing heritage often lacks a social and historical context. This problem may be endemic to Birnbaum's thesis on the mythical nature of traditional anti-Semitism, which continues to confront Jewish cap-

italists and revolutionaries regardless of their real status in society, yet it undermines at times the impact attributed to those references.

Alongside traditional anti-Semitism, Birnbaum detects a new "political antisemitism" born of the Third Republic and rooted in a "denunciation of the strong state which imposes its universalism and equal dignity of each citizen while favorising at the same time the precocious emancipation of the Jews" (p. 302). Absorbing the intense hatred of certain sectors against the state's republican ideals, Jews came to symbolize all that was depraved in the republic. They alone were not singled out in this process, but the anathema heaped on the "republican Jews" in the embodiment of Blum, Mendès-France, Abraham Schrameck, and Georges Mandel was unsurpassed. Deriding their universalistic ethics and efforts to further a secular ideology bent on uprooting all that was cherished in prerepublican French society, diverse personalities from Charles Maurras to Pierre Poujade raged at the Jewish influence over the state. The Jews were perceived as mobilizing the state's apparatus for control of the French mind and ultimate domination of France. Here, too, Birnbaum leaves few stones unturned, revealing the limitations of studies that minimize anti-Semitic opposition to Jewish political leadership in France, but here, too, one wants further insight into the contextual significance of the Jews' rejection from the state structure.

Rich in detail and originality of thought, this book presents a convincing argument on the staying power of anti-Semitic thought in French society, in its traditional and political guises, while raising a stirring challenge to the French political system, just as Sartre's essay did over a generation ago.

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MICHAEL S. ROTH. *Knowing and History: Appropriations of Hegel in Twentieth-Century France*. Ithaca: Cornell University Press. 1988. Pp. xv, 264. \$28.50.

This *explication de texte* of the major works of Jean Hyppolite, Eric Weil, and Alexandre Kojève will be welcomed by specialists interested in the renaissance of Hegelianism in France during the 1930s. Michael S. Roth is sure-footed as he scales the slippery slopes of these three philosophers' writings, which are no less elusive than Hegel's own. His exegetical method skillfully combines paraphrase and metaphor to yield a textual analysis of remarkable clarity and precision. The reader is treated to a sensitive commentary on Hyppolite's "Heroic Hegelianism" and its confrontation with "unhappy consciousness," Weil's passionate plea for mankind's replacement of dialogue for violence in the search for truth, and Kojève's complex rendering of the master-slave dialectic.

The book will probably prove less useful to intellec-

tual historians concerned with the broader range of cultural developments in twentieth-century France because of its rigorously internalist method of analysis. Roth tends to treat his three writers in a vacuum without attempting to situate them in their social, cultural, and institutional milieu. Apart from occasional references en passant to concrete historical developments, one learns very little about how the momentous political crises and cultural transformations in France from 1930 to 1968 affected their reading of Hegel. The author's scrupulous attention to the nuances of the texts appears to have distracted him from an appreciation of the context within which these men lived and wrote.

It could be argued that the lucid exposition of recondite philosophical writings bears its own justification, thereby releasing an author from the obligation to establish the connection between the subject of his or her study and its broader sociocultural setting. An explication of Plato's *Republic* or Freud's *Interpretation of Dreams* that significantly enhances our comprehension may be entitled to give short shrift to the historical particularities of ancient Athens or nineteenth-century Vienna. But such an argument loses much of its force when advanced on behalf of writers whose work is largely derivative rather than original. With the arguable exception of Kojève, the three subjects of Roth's study cannot be counted as seminal thinkers. Even as interpreters of Hegel, they offer few insights that had not been anticipated by earlier commentators. Nor do they qualify as influential founders of a new philosophical school; the Hegelian revival in the 1930s never amounted to more than a small, isolated, ephemeral cult in France, which was soon engulfed by existentialism, poststructuralism, postmodernism, and other postwar Parisian intellectual fashions. Not even its three standard-bearers remained faithful to the cause for very long. Hyppolite and Weil abandoned the master for Heidegger and Kant, respectively, while Kojève retreated to a bleak nihilism that reduced his philosophical activity to pondering the implications of the end of history in his own time.

In an afterword, Roth makes the important point that the French Hegelian tradition did exercise a powerful, if entirely negative, influence on the thought of Michel Foucault. Foucault's discovery of Nietzsche led him to reject the Hegelian search for a uniform meaning and direction in history in favor of a Nietzschean appreciation of the discontinuities, fragmentation, and pluralism of human existence. But one is left to wonder whether Foucault's "replacement of a Hegelian approach to the past with a Nietzschean one" (p. 221) was prompted by his exposure to the commentaries of Roth's trio or by his confrontation with the works of Hegel himself.

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JONATHAN BUCHSBAUM. *Cinema Engagé: Film in the Popular Front*. Urbana: University of Illinois Press. 1988. Pp. xiv, 308. \$29.95.

Jonathan Buchsbaum, a professor of communications, has undertaken the difficult task of tracing the development of independent political cinema during the era of the French Popular Front. Unlike many non-historians, he has thoroughly examined the historiography of the period, so that his book contains few factual errors.

Buchsbaum describes the efforts of left-wing parties and organizations to create a cinema liberated from the constraints of the film industry and censorship. He naturally focuses his account on *La Vie est à nous*, which was prepared by a group of prominent artists and intellectuals, including Jean Renoir, as a cinematic electoral platform for the French Communist party's 1936 campaign.

Buchsbaum explores the relationship between film making and politics. His main point, in formal terms, is that *La Vie est à nous* and other films juxtaposed documentary sequences with fiction not so much to lend authenticity as to "combat the conception of the newsreel image's supposed affinity with truth" (p. 101). Renoir's *La Marseillaise* of 1938, intended to be the culmination of the new cinema, instead marked its decline. The film's apolitical treatment of the French revolution reflected the disintegration of the Popular Front coalition. Moreover, the film reverted to the very costume drama that independent cinema had challenged in the first place.

This book has two limitations. The first is that its thoroughness in both text and footnotes makes it less than amiable reading. The second is its sentimental attitude toward the French Communist party (Parti communiste français [PCF]), including occasional dithyrambic passages about the PCF's role. Buchsbaum writes, for example, "Such foresight—more than merely fortuitous—provides powerful evidence that the PCF did speak for the working class, did understand their concerns, and thus confirms its claims to have been the prime animator behind the creation of the Popular Front in response to an inchoate popular will that the PCF was uniquely placed to translate into concrete action" (p. 83). Such a questionable idealization of the PCF should not prevent the reader from patiently culling useful information from this work.

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BERNARD PUJO. *Juin, Maréchal de France*. Paris: Albin Michel. 1988. Pp. 407. 150 fr.

Bernard Pujó served on Alphonse Juin's staff from 1953 to 1959. Predictably, therefore, this is a sympathetic biography, based partially on personal recollections and partially, too, on the canons of historical scholarship.

Although Pujó made significant use of the French

national, military, and diplomatic archives, his research is far from complete, and the bibliography is similarly abbreviated. Foreign archives remain foreign, and, despite Juin's role in wartime diplomacy and postwar international relations, this volume relies exclusively on French-language sources. Finally, the author is further constrained by his dependence on memoirs at the expense of interpretive works proffered by historians. Accordingly, one might never know when, or even whether, Pujo is saying anything interpretively novel about Juin.

Yet rarely does a book impoverish, and in this case the rewards of reading this volume are substantial. Pujo's book is a full life of Juin, from his birth in Algeria in 1888 to his death in Paris in 1964, a death surely hastened by his split with Charles de Gaulle over the issue of Algerian independence. The seventy-five year interim that was the life of Juin is bound to attract anyone interested in the history of modern France and in military history, to be sure, particularly of the sort likely to include Juin's Italian campaign in 1944. Scholars of political and international history will also be interested in Juin, for he was among the senior officers in French North Africa who, in 1942, had to choose between Pétain, the commander-in-chief, and the bewildering option offered by Franklin Roosevelt and Winston Churchill, Henri Giraud and de Gaulle. Juin became a central figure, one whose counsels were instrumental in limiting the bloodshed during the Allied landings in North Africa and one who, conversely, was eager to commit French forces against the Germans who had arrived unbidden in Tunisia.

But fighting the German army during the war, or the presumptions of Anglo-Saxon allies afterward, proved easier than resisting the forces of decolonization in Indochina, Tunisia, Morocco, and Algeria. Elevated to the honor of *maréchal* in 1952, Juin became a permanent military adviser to the Fourth Republic. But not everyone listened, least of all the government. Cabinet after cabinet, he complained, failed to head off the nationalists in the Far East and the Maghreb, failed to outflank by reform and contain by force. Retired in 1956, Juin watched in anguish as the republic lost its will and France its empire. With the Algerian crisis, the marshal became a public critic of government, including that of the returned de Gaulle. He felt betrayed and said so with characteristic bluntness for which de Gaulle vowed to "bust" him. Yet Juin never advanced from the word to the deed of rebellion, never joined and refused to lead the conspiracy of generals against the state.

By Pujo's telling, the man within the marshal was no plotter. When deadly earnest, Juin was terse and brutally candid, particularly before those disinclined to listen. But his customary manner was far from grave; he was a man of abundant if sometimes coarse humor, a hunter and equestrian, a passionate bridge-player and reader of Barrès and Proust. There is not much of this side of Juin in Pujo's volume, but there is just enough of the marshal at leisure, at the Académie

Française, and at home as husband and father to put some flesh on the bones of this modest man of immodest accomplishment.

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MICHAEL E. WILLIAMS. *St. Alban's College Valladolid: Four Centuries of English Catholic Presence in Spain*. New York: St. Martin's. 1986. Pp. xiv, 288. \$35.00.

Readers of Michael E. Williams's other work, *The Venerable English College Rome: A History, 1579-1979* (1979), will expect writing and research of a high caliber. The present volume, a companion piece to his earlier work, fulfills that expectation. Written as a general history in readiness for the fourth centenary of the college, this book is an examination of the history of the college from its founding in connection with the other seminaries of the English Catholic community at Douai (1559) and Rome (1579) through its importance for the deliberations of the English Catholic hierarchy during the Spanish Civil War and its role in Britain's effort during World War II.

In the first part of the book (chapters 1 through 5, a short seventy pages), Williams discusses the college under the care of the Jesuits from 1589 until the suppression of the society in 1767. He indicates the importance of the strategic geographic location of the college between Rome and England during the French wars of religion, its freedom from civil disorder, and its fostering of an atmosphere supportive of study and prayer. After initial difficulties, the common religious link of Catholicism overcame political hostilities between England and Spain, and the college clearly profited from the material support of the people. The Spanish Jesuit provincial made decisions about the college's confessors and spiritual directors, a Spanish Jesuit served as rector, and the students attended classes at the local college of the society. St. Alban's presence in the middle of a Catholic country and its ties to the Jesuits clearly shaped it as an enclave of popular Counter Reformation Catholicism and kept alive in the students the ideal of a medieval Catholic England.

After the suppression of the Jesuits in the Spanish dominions, St. Alban's came under the control of the secular clergy. Chapters 6 through 12 (pp. 71-227) of Williams's study cover the relationship between the college and the European Enlightenment, the vagaries of several generations of administrators (most of the surviving historical materials are of an administrative nature), and the impact of the changing Spanish political scene on the financial stability of the school. Struggling with the Enlightenment and regalism, the college clung tenaciously to its heritage and seems to have kept alive the "age of the martyrs" and the ideal of a Catholic culture. The author concludes the work with a brief chapter on the history of the college from World War II to the present and six very helpful appendixes: a noteworthy one including documentation on every-

day life in the college, a description of the archives, a listing and portraits of the college martyrs, a chronological list of the rectors, and two short expositions on St. Gregory's College, Seville, and the residence house at Sanlúcar de Barrameda.

This work sheds great light on a little-known but important link in the seminary system of post-Reformation English Catholicism. The book is significant for its placement of the seminary system in the context of general political history and its description of the complexities of ties between England and Spain from the Elizabethan period to the present. Although the author concentrates on administrative and financial developments, he makes important comments on theology, teaching, prayer life, and the English Catholic ethos. Williams is to be congratulated for his interesting and solidly researched work.

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FERNANDA ROMEU ALFARO. *Mas allá de la Utopía: Perfil histórico de la Agrupación Guerrillera de Levante*. Foreword by ANTONIO M. CALERO. (Estudios Universitarios, number 28.) Valencia, Spain: Alfons el Magnánim. 1987. Pp. 219.

The guerrilla struggle against Francisco Franco, which continued for more than a decade after the end of the Spanish Civil War, is an important and much neglected topic. It is also, by its nature, a difficult one to uncover. In this book Fernanda Romeu Alfaro has attempted to "retrieve the historical space" (p. 14) of the largest of the guerrilla forces, and she has done a superb job of research. Most impressively, she was able to breach the walls of the archives of the Civil Guard, the paramilitary police charged with suppressing the guerrillas. She has also used the archives of the Spanish Communist party, which provided material support, organization, and political direction for the Agrupación Guerrillera del Levante (AGL), and she conducted interviews with forty-six participants, including former members of the Civil Guard.

Unfortunately, the author is incapable of doing much with her material, and her good intentions and first-rate research have largely gone to waste in this very disappointing book. It is, to begin with, terribly organized. After a skimpy and disjointed background chapter come chapters on the role of the Civil Guard, on the "sociología" of the AGL (of which only a small part is anything like a sociological analysis), and, finally, on the history of the AGL and its activities. It would have been much more logical and certainly much easier on the reader, even a reasonably well-informed one, if the order of chapters two through four had been reversed.

The author's inability to digest or analyze her material is evident in the repeated insertion of long lists like catalogues in the midst of the text. For example, pages

150 to 152 contain nothing more than a string of sentences listing the actions carried out by the AGL in 1945 and 1946. Pages 157 to 163 contain another such catalogue, this one for the period 1947-48. The author also tends to do the same with excerpts from her interviews. Perhaps the root of the problem was uncertainty over just what type of book to create, an oral history or a more traditional one. The present book straddles the two rather uncomfortably. Alfaro seems to have had sufficient material to have been able to produce an oral history of guerrillas in eastern Spain in the style of Ronald Fraser. The product would almost certainly have been better had she done so.

The reader learns things that are interesting and useful, such as the experiences of four women who actually participated in the guerrilla struggle, but one does so as much in spite of what the author has done with her sources as because of it.

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CRAIG E. HARLINE. *Pamphlets, Printing, and Political Culture in the Early Dutch Republic*. (International Archives of the History of Ideas, number 116.) Boston: Martinus Nijhoff. 1987. Pp. xiv, 309. \$78.50.

The considerable body of pamphlet literature produced in the Dutch Republic has in the past been used either as illustrations of the history of political thought or as evidence of public opinion with regard to some particular incident or topic. This book is the first attempt to give a systematic account of the whole body of that material over an extended period, 1565 to 1648, and to attempt to relate its production to political and social circumstances. Because of the great number of pamphlets produced, Craig E. Harline concentrates on a random sample of five hundred taken from the catalogue of the major Dutch collection, giving subsidiary attention to a selected group of best sellers, which are defined arbitrarily as those with four or more printings. Both those groups are then subjected to computer analysis for a wide range of variables, both for the whole period and for the periods before and after 1606.

What emerges from those analyses is more a confirmation of existing general impressions than anything particularly new or startling, but that in itself is a useful contribution. Whether Harline's approach is helpful with regard to some major questions may be doubted, however. For example, the best-selling pamphlets are shown to have been unanimously in favor of both the prince of Orange and the states of Holland in the period before 1606, whereas they were sharply divided after that date. Writing in favor of the prince or the states before 1606, however, was a simple patriotic statement, whereas afterwards it was a partisan political position, so the terms used had changed their meaning. The suspicion lingers that multivariate analysis does

not work very well in cases like this, where the variables, however important, tend to be both slippery and chameleon-like even over quite short periods.

The author's treatment of the political and social context of the production, distribution, and consumption of pamphlets is useful, particularly in his emphasis on the attempts of the various authorities in the republic to suppress unwelcome opinions. Freedom of the press was much more restricted in the Dutch Republic than is often recognized, at least in English-language treatments of the period, and the wide range of opinions put forward in the pamphlets dealt with here was a consequence of the failure, not the lack, of repression. The picture that emerges of writers, pamphlets, and their audience is an interesting one, but rather because of the author's descriptions of and quotations from individual pamphlets than because of his sophisticated computer analysis. It would have been useful if the study had been continued to cover the third quarter of the seventeenth century, which was probably the most prolific period of Dutch pamphlet production; indeed the study might have gained in coherence if the revolt section had been dropped and the terminal date moved to about 1672. This book has the characteristic strengths and weaknesses of the doctoral dissertation (although there is no indication that it is one), but in the end it would be churlish not to welcome a competent study of an interesting and neglected topic.

J. L. PRICE
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RUNE JOHANSSON. *Small State in Boundary Conflict: Belgium and the Belgian-German Border, 1914–1919*. (Lund Studies in International History, number 24.) Lund, Sweden: Lund University Press. 1988. Pp. 240. 125 KR.

Territorial acquisitions and disputes have frequently been studied with great profit by historians. One such case study that has escaped serious and complete examination and scholarly analysis is the Belgo-German border controversy over Eupen and Malmédy during and after World War I. In a forcefully stated and excellent account, Rune Johansson remedies much of this historical deficiency with his detailed and dispassionate book. Other works, such as those of Sally Marks and Jonathan Helmreich in English, those of Horst Lademacher and Heinz Doepgen in German, and especially those of Jacques Willequet in French, have explored this topic, but no other scholar has asked fully the important and difficult questions about the pre-Versailles period. Johansson's treatment has combined both depth and perspective with a sharp focus on the emergence of the Belgian annexation policy.

One comes away from this work with a distinct feeling that there are rewarding observations made and that they are based on formidable research in the vast and complex Belgian materials. Johansson comprehends the frame of mind of policy makers in small

nations, for he is obviously well versed in the subject of Belgian politics as they related to the problem of *Wallonie malmédienne*. He sees the essence of this problem in the divergences within Belgium as Belgians argued over expansion of the eastern frontier. The split into those who opposed and those who supported transfer of territory and population groups, with the ethnically based interests in this bicultural state mixed with strategic and security considerations as rationales for acquisition, resulted in divisiveness and not national cohesion. Although the nations that met at Versailles did not support the Belgian claims, Johansson is correctly more focused on the nature and meaning of annexation in Belgium and its consequences. His effective argument puts forth an ultimate "cancelling out thesis," whereby Flemish opposition and Walloon advocacy, Centrist support and Socialist negativism invalidated any concerted and successful venture to gain the borderlands with either full Belgian agreement or Allied support.

This volume, based on the major archival sources on the topic, contributes a convincing portrait of the border area for Belgians during wartime and the peace-making era. Although much time and space is spent on the non-elite groups and their views on annexation, the author centers his study on the political parties, vested interest groups, and ministerial decision makers. In the end, the Belgian border policy was seen by those leaders as "giving the country economic and military strength as a guarantee for its independence and security" (p. 146). The major impetus for the aggrandizement policy of the national leaders was, therefore, the enhancement of the national interest, strength, and postwar status. Again the ethno-national factor appears writ large throughout this work, but Johansson argues (much less effectively) that Flemish-Walloon differences mattered less in the long run in the ultimate national decision to make the claim a major priority.

The book could have used more thorough editing and is at times not especially readable. Some parts are heavy going, with wearisome and questionable detail (pp. 74–78). Some evidence seems contradictory with diversity and homogeneity of groups implied at the same time (pp. 94–96, 151). There is, however, more than flashes of innovative insight in this competent work, mostly the linkages of foreign and domestic policy. Taken *in toto*, the book is, in spite of these shortcomings, certainly an instructive account and a well-considered interpretation.

PIERRE-HENRI LAURENT
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KARI TARKIAINEN. *Se vanha vainooja: Käsitykset itäisestä naapurista Iivana Julmasta Pietari Suureen* [The Devil: Interpretations of the Eastern Neighbors from Ivan the Terrible to Peter the Great]. Summary in German. (Historiallisia tutkimuksia, number 132.) Helsinki: Suomen Historiallinen Seura. 1986. Pp. 349.

RAIMO RANTA. *Viipurin komendanttikunta 1710–1721: Valtaus, hallinto ja oikeudenhoito* [The Viborg Command Post, 1710–1721: Conquest, Administration, and Jurisprudence]. Summary in German. (Historiallisia tutkimuksia, number 141.) Helsinki: Suomen Historiallinen Seura. 1987. Pp. 479.

These two books deal with Swedish-Russian relations and Swedish and Finnish views of Russia in the early modern period, but they largely explore different kinds of issues. Kari Tarkiainen's work has grown out of his published doctoral dissertation and includes much of the same material. Raimo Ranta's study complements his earlier extensive work on southeastern Finland.

In describing Swedish and Finnish views of Russia from the sixteenth to the early eighteenth centuries, Tarkiainen also discusses their origins but does not try to establish their validity. He traces many of the origins of these views to the Baltic Germans and points out connections between Swedish and general contemporary Western notions of Russia. The image of Russia that emerges is overwhelmingly negative. Russians are described as cruel, sadistic, violent, bloodthirsty, treacherous, and amoral, as witnessed both by their methods of waging war and by conditions in Russia. Detailed descriptions of mutilations of men, women, and children by Russian troops, the use of units specializing in terror and destruction, and domestic cruelties amplify these views.

The Swedes also viewed Russians as simple, crude, uncouth, filthy, smelly, and unreliable people who lacked education, manners, and knowledge of foreign languages, made faces and body sounds in public, engaged in freely tolerated sodomy, homosexuality, wife beating, and excessive drinking. The descriptions of public drunkenness of men and women, monks and nuns, to the point of stupor reflected also difficulties that the Swedes had in dealing with Russians, who were offended by foreigners who declined a drink. The most thorough and interesting Swedish observations were based on years of residence in Russia.

In addition to observing the Russians as people, the Swedes drew conclusions about the nature of the Russian state, laws, military power, and religion. In their view Russia was essentially a tightly controlled slave and police state where people accepted persecution and suffering and were kept in check through internal spying, informing, and excessively cruel penalties from which no one was safe. This system raised obstacles to contacts between Russia and foreigners. It prevented the Russian government from sending young Russians abroad to study for fear that the attraction of freedom would encourage defections. Only diplomats, spies, and some merchants bound by written oaths and securities could visit abroad. The system suppressed initiative for which the Russians suffered in their conduct of war. Inflexible following of the tsar's orders kept the Russian commanders from

effectively exploiting their vast resources and opportunities in the shifting conditions of fighting.

The Swedes noted vast improvements under Peter the Great in science and foreign language education, engineering, and the military, but they also noted neglect of ethics and history. Observing a growth of cynicism among the Russian elite after exposure to the West, which resulted in a loss of faith in Orthodoxy, the Swedes remained skeptical of the depth of Russian changes.

Tarkiainen explains the negative Swedish views of Russia as a result of their partial origin in propaganda and the profound differences in culture and the political and social structure between the two societies. The argument that hostile propaganda is an explanatory factor is weakened in part by similarities in contemporary views of Russia among Western societies with no tradition of enmity toward the country.

Raimo Ranta covers a much shorter period than does Tarkiainen. He concentrates his study on the second half of the Great Northern War known as the Great Wrath in Finnish history. After introducing the background of the Russian invasion of southeastern Finland, Ranta discusses the nature and conduct of the military operations and Russian occupation. He devotes roughly the same amount of space to a study of the Russian administrative and judicial system in the occupied territories. The Great Wrath left many negative memories. Descriptions of horrors suffered at the hands of the Russian occupiers were passed on from generation to generation and were reinforced by deliberate propaganda. The conduct of the invading Russian troops and Tsar Peter the Great himself confirmed traditional Swedish and Finnish views of Russian cruelty and treachery. Repeatedly breaking pledges, the tsar and his commanders lived up to the Russian reputation for untrustworthiness as partners to agreements. Use of torture against the population by Russian troops to discover hidden supplies and valuables and to force cooperation spread terror. Rumors and generalizations based on particularly gruesome incidents further demoralized civilians and caused them to flee. Another result of the Great Wrath was the emergence of guerrilla warfare.

The Russian administrative and judicial system sought collaboration from the few remaining Swedish and Finnish officials and clergy through a system of coercion that was totally beyond their experience. This treatment also drove home the traditional notion of Russia as a vast slave state. If they did not cooperate in collecting taxes for the Russians, the officials, including the clergy, were liable to a humiliating physical punishment by flogging. It was no consolation that Russian officials themselves were liable to similar punishments. Ranta also discusses briefly efforts by Peter the Great to modernize the Russian system of administration and justice by following Swedish models.

Having done prior significant work in their topics, both Tarkiainen and Ranta know their sources well.

They have made welcome contributions to our knowledge of north European history.

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BO HAMMARLUND. *Politik utan partier: Studier i Sveriges politiska liv 1726–1727*. [Politics without Parties: A Study in Swedish Politics in 1726–27]. Summary in English. (Acta Universitatis Stockholmiensis, Stockholm Studies in History, number 35.) Stockholm: Almqvist och Wiksell. 1985. Pp. 248.

GUNNAR ARTEÚS. *Krigsmakt och samhälle i frihetstidens Sverige* [Military and Society in Sweden during the Liberty Era]. (Militärhistoriska Studier, number 6.) Stockholm: Militärhistoriska. 1982. Pp. 437.

Bo Hammarlund's work examines political activity in Sweden in 1726 and 1727 to determine the origins of the political parties and intense parliamentary activity usually associated with the later decades of the Age of Liberty, the intriguing period of Swedish history dating from the death of Charles XII in 1719 to the restoration of royal absolutism by Gustav III in 1772. The major political issue of 1726–27 was that of adherence to the Hanoverian alliance extended to Sweden by both England and France. Accession to it amounted to a rejection of Russian influence and also of the interests of Duke Karl Fredrik of Holstein-Gottorp. He was a Russian favorite who had lost his lands to Denmark during the Great Northern War by supporting Sweden; he was also the son of Charles XII's older sister. He had therefore been passed over in the succession after Charles's death in favor of the king's younger sister, Ulrika Eleonora, and her husband, Fredrik I.

The work is not a chronological narrative of the stormy resolution of this issue but is, instead, a study of several episodes illustrating the nature of the political groups existing in Sweden at the time and their means of operation. It examines particularly the methods employed by Count Arvid Horn, the head of the Royal Council, to eradicate the Holstein party that opposed the Hanoverian alliance and supported Duke Karl Fredrik.

In his study of party activity, Hammarlund discusses Horn's maneuvering within the council and the Riksdag of 1726–27, called to consider the treaty, the legal and parliamentary action taken at Horn's instigation against leading members of the Holstein faction, the degree of foreign influence in the question, the propaganda campaigns undertaken by both sides regarding the alliance, and appointment policies of the government in 1727 that clearly favored Horn and the pro-alliance faction after the diplomatic issue had been decided in their favor. Hammarlund concludes, as the title of the book suggests, that Sweden during the 1720s can be characterized as possessing politics with-

out parties. In other words, the party system associated with the Hats and Caps had not yet evolved during the decade. The author contends that Sweden's political system was certainly much different from what it had been under the absolutist Charles XI and Charles XII. The struggle over the alliance and the Riksdag session of 1726–27 also contained significant elements of mid-century Swedish politics, such as the emphasis given to political tactics, planning, and political mobilization by both the council and the Riksdag, the use of foreign subsidies, and the politicization of the Riksdag itself. Hammarlund also concludes that Horn ultimately paid for his role in the events of 1726–27, for, by increasing the role of the Secret Committee of the Riksdag in order to vanquish the Holstein party, he paved the way for his own political destruction by the same body a decade later.

Although Gunnar Arteús's work addresses in a general sense the indices of the militarism existent in any society, particularly preindustrial states with sophisticated forms of state administration, he is addressing very specifically the militarization of Swedish society during the Age of Liberty. By this he means the degree of impact that the military system had on the social, economic, and political structure of eighteenth-century Sweden.

Arteús concludes that political life and government were significantly "militarized" because about one-half of the holders of major offices, almost without exception noble, were also former military officers. The Swedish economy may also be considered to have been militarized, given the fact that the peacetime expenditures of Sweden's military forces consumed 52 to 69 percent of the royal budget, and personnel costs accounted for a lion's share of the figure. Socially, the order of precedence followed throughout most of this period as well as access to public honors illustrate the social prestige accorded this caste as well.

This work is significant not only for its consideration of these questions but also for the light it sheds on other subjects. The author's research into the economic aspects of the issue points out the importance of the *indelningsverk* on an officer's pecuniary position. This institution consisted of the rent and tithe payments received by military officers of the cavalry and country (*län*) regiments. In an age of rapid inflation, particularly of agricultural products, positions in these units were preferred to posts in military units paid in cash. Not surprisingly, such regimental units possessed a much higher proportion of noble officers than did other military units.

This situation also demonstrates the increasing dependence of nobles on governmental service for their livelihood after the great *reduktions*, or land confiscations from the nobility in the late seventeenth century by King Karl XI. As Arteús notes, the nobility were therefore well placed to defend their interests when these were threatened during the eighteenth century.

The work itself contains extensive statistical data to support the author's arguments, particularly a section

regarding the social recruitment of the army's commissioned officers. In some ways Arteús's conclusions seem understated because he hesitates to declare that all of Swedish society was militarized by these developments, but he does conclude convincingly that the leadership of that society was profoundly militarized during the Age of Liberty. The author is an acknowledged expert on the subject, and the work is representative of a significant effort on the part of many Swedish historians, such as Björn Asker and Kurt Ågren, to study the influence of the military (and particularly the nobility within the military) on the Swedish state and society during the seventeenth and eighteenth centuries.

These two works approach the Age of Liberty from different angles, but they add much to our understanding of the forces at work within a turbulent and important period of Swedish history.

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H. ARNOLD BARTON. *Scandinavia in the Revolutionary Era, 1760–1815*. (Nordic Series, number 12.) Minneapolis: University of Minnesota Press. 1986. Pp. xii, 447. Cloth \$39.50, paper \$16.95.

"It is hard to discern a more momentous half-century in the entire history of the North." This final statement in H. Arnold Barton's book on Scandinavian history from 1760 to 1815 is well founded. During this time, the social structure of Denmark was radically changed, as were the constitutional and, to a lesser extent, social structures of Sweden. In 1809, Finland was separated from the Swedish kingdom, and, in 1814, Norway was separated from Denmark and, as an independent kingdom, united with Sweden.

Barton himself has strong ties to Sweden through ancestry and marriage, and he has made repeated, and lengthy, visits to the country. But his education is American, and he has been especially influenced by his teacher R. R. Palmer and Palmer's magnificent work *The Age of the Democratic Revolution* (1959). During Barton's own research—including, among other things, the biography of the Swedish aristocrat Hans Axel von Fersen—he found that little has been said about Northern Europe in general histories, and what has been said has all too often been erroneous (p. 359). He has also noticed that Scandinavian historians have tended to write for each other, or at least for their compatriots (p. x). This is true, although there are exceptions (for instance, the chapter "Der Norden 1720–1815" in *Historia Mundi*, 9 (1960): 172–96).

Barton knows Scandinavian historiography better than many Scandinavian historians. It is hard to find errors in his book. Minor mistakes, however, do exist—for example, *Kammerkollegium* instead of *Kommerkollegium* (p. 54), *Kammerskollegium* instead of *Kammerkollegium* (p. 64), Johan Fredrik instead of Johan Henrik (p. 176), Gunnar instead of Axel (p. 412). Barton's style is fluent, sometimes brilliant, and his com-

parative approach is decisive. He is continuously looking for inter-Scandinavian and European parallels and contrasts. Like Palmer, he finds an almost rhythmic change between more "conservative" and more "progressive" periods. Both the American and the French revolutions influenced Scandinavian developments, but Barton stresses that many important Scandinavian reforms were initiated, mostly "from above," before 1776 or 1789 (p. 383). Scandinavian historians may have often been too modest in their comments on this issue.

Barton maintains a good balance in his treatment of the Danish-Norwegian and the Swedish monarchies and pays due attention to their "outer" parts: Schleswig, Holstein, Norway, Iceland, the Færø Islands, Finland, and Swedish Pomerania. Like many other historians, especially those in Finland, he uses the term "Sweden-Finland" for the old Swedish monarchy. This usage is practical and pedagogical but anachronistic, because Finland was, as Barton says himself, a part of "a single, unitary kingdom" (p. 32), not a state of its own. As a matter of fact, the division between the central provinces, including Southwestern Finland, and the more peripheral ones was often wider than that between "Sweden proper" and Finland.

In dealing with classical controversies, such as the meaning of the Swedish "Era of Freedom" (1718–72), the great Danish reforms after 1784, and the Swedish "policy of 1812," Barton is eager to see the points of diverging sides. He emphasizes the roles of leading personalities (p. 357). The Danish statesman A. P. Bernstorff is especially respected. Like many other German-born Danish politicians, Bernstorff had a finer sense of the needs of the people than had many Danish-born colleagues. Gustaf III of Sweden, a complex figure, is regarded in a rather positive way (compare p. 105). The two big "losers" in Scandinavian politics during the Napoleonic wars, the cousins Frederik VI of Denmark and Gustaf IV Adolf of Sweden, are, for all their inadequacies, treated with some understanding.

Barton has fulfilled his great task with an admirable knowledge, breadth of perspective, talent, and prudence. He deserves the title "the Swedish American of the year," awarded to him in his ancestors' country in 1988.

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HEINZ SCHILLING. *Aufbruch und Krise: Deutschland 1517–1648*. (Das Reich und die Deutschen.) Berlin: Siedler. 1988. Pp. 507. DM 98.

In this enormous and beautifully produced volume, Heinz Schilling addresses himself to the general reader in the Federal Republic of Germany and seeks both to discredit many traditional views of the early modern period and to explain new theories that historians have developed over the last twenty years. This work is part

of a new series on German history published by Siedler, a series that covers the entire span of German history from the end of the Roman empire to the present; Schilling also wrote the subsequent volume, *Vom Alten Reich zum Fürstenstaat: Deutschland 1648–1763* (1984).

Although the book is written as a general survey and covers the entire period thoroughly, it still has a particular perspective. Schilling is primarily interested in tracing two developments: those forces and events in German history that led to political and economic modernization, and revolutionary or democratic movements. Both have been concerns of Schilling's in previously published work, but readers unfamiliar with his ideas may be surprised at his analysis. For example, unlike many historians who regard the development of rigid confessionalism in late sixteenth-century Germany as hindering progress toward the modern, secularized state, Schilling views it as assisting princes in their attempts to control all aspects of their subjects' lives. Rather than slowing down the growth of absolutism, confessionalism contributed to it. Schilling's ideas about early modern revolutionary movements are even more revisionist. He does not view the sixteenth and early seventeenth centuries (or what he terms the "long sixteenth century" [p. 54]) as a period of unchallenged growth of princely power but as a time of frequent revolts by peasants and urban residents. Those revolts were not futile or anachronistic, in Schilling's opinion, but important in shaping the early modern state.

At times Schilling's concern with modernization seems a bit forced, but his revisionist challenges to many long-cherished ideas are refreshing and convincingly argued. Not only does he make a good case for the continued political activity of peasants long after the Peasants' War but he also refutes the notions that Lutheranism always made one more obedient to authority than Calvinism, that the Peace of Augsburg made the empire weaker, and that urban representative institutions stood in opposition to the growth of centralized princely power. Maximilian I of Bavaria—who since Leopold von Ranke has generally been considered a nervous and superstitious ruler, overly influenced by the Jesuits—emerges instead as the creator of the most modern and strongest territorial state in the empire. In all of those revisions, Schilling draws on his own extensive archival research as well as that of other contemporary historians, whom he often mentions by name in the text. His discussions of points of contention and alternative hypotheses provide general readers with a sense of the historiography of the field, which is usually lacking in such surveys.

Readers familiar with Schilling's previous work (a number of his articles have been translated and have appeared in English-language journals and collections) will not be surprised that he stresses the connections between religion and politics nor that he does not see 1555 as marking much of a significant break in German history. Particularly when discussing intellectual and economic developments, he views events in Germany from a wider European perspective. Much of the book

is fairly traditional political and military history, although he does include several subchapters on the resulting cultural and social changes, including the witch craze and the upsurge in anti-Semitism. What is missing is much consideration of what might be termed micro-social history, such as research on the family, women, or popular culture. This lack may partially result from the way the entire series is framed and from the already extremely broad scope of this particular volume. The broad time frame allows Schilling to explain many long-term changes, including the very perplexing question of how the nobility was able to reassert its dominance of society in the seventeenth century, after a century dominated both ideologically and economically by the middle class.

The book includes hundreds of illustrations and is printed on such fine quality paper that even the smallest details of woodcuts and engravings are easy to see. Each illustration is not only clearly identified but also discussed briefly, which enables the reader to see how each fits into the story related by the text. At a time when the history books published by American publishers are generally either textbooks for students or coffee-table histories for the general public, a volume like this is a refreshing change. One can only wish that the market for general histories of this sophistication existed on this side of the Atlantic.

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MICHAEL STOLLEIS. *Geschichte des öffentlichen Rechts in Deutschland*. Volume 1, *Reichspublizistik und Polizeywissenschaft 1600–1800*. Munich: C. H. Beck. 1988. Pp. 431. DM 98.

This is the first of two volumes by Michael Stolleis on the history of the intellectual discipline (*Wissenschaft*) of "public law" (*öffentliches Recht*) in Germany. Most work on the history of the Roman law tradition in Western Europe has been preoccupied with private law, largely because that was the principal focus of the legal profession in the medieval and early modern periods. This preoccupation with private law also resulted from the fact that central concepts of thought concerning the state in the Western tradition were improvised from late Roman doctrines of private law, particularly those concerning wardship and testament. The great exposition of this development is now Helmut Coing's massive *Handbuch der Quellen und Literatur der neueren europäischen Privatrechtsgeschichte*. Stolleis writes his own history with one eye on that enterprise, but he makes no attempt to create a public law analogue to Coing's manual.

What he does do is describe the birth of a university discipline and its development until the demise of its principal subject, the Holy Roman Empire of the German Nation. That story is known to English-speaking readers through Hanns Gross's *Empire and*

Sovereignty (1974), but Stolleis has profited from the expansion of knowledge in the last two decades about early modern thought on the state. Germany contained many states, but there was always a unified legal profession. Despite the ramshackle nature of the *Reich*, public discussion continued to be carried out in terms of *Reichspublizistik*, which was the special province of public law. Although the public law specialists fed on the Roman law tradition and republished the works of medieval writers such as Marsiglio of Padua, the discipline did not come into its own until after 1600. A total of forty universities produced jurists who served as judges, advocates, and administrators in the various states. They contributed to a "juridification" (*Verrechtlichung*) of public life that has left traces in German political thought to the present day. The empire with which this discipline dealt was very different from the despotism described in the *Corpus iuris civilis*, and the failure of both Reformation and Counter-Reformation meant that German Central Europe had to accommodate several licit religions within one polity. The necessity to describe and defend this strange construct forced a type of historical awareness on public jurists, resulting in their development of a special study of *Reich* history, which prepared the way for history as a subject in its own right in universities. The principal writers in the public law tradition were Protestants, although there is no obvious reason for this correspondence, and a disproportionate number of the better writers were Calvinists.

Far from being isolated, public law teachers and writers continued to be sensitive to the intellectual trends of the whole of Europe. The fact that the universities provided the chief setting for discussion meant that Latin was the primary vehicle, with German entering the scene late in the seventeenth century. Machiavelli, Hugo Grotius, Jean Bodin, Spinoza, Thomas Hobbes, and Montesquieu all made their appearance in the literature, if only as devils to be denounced. Major writers within the German tradition included Hermann Conring, Johannes Althusius, Samuel Pufendorf, Gottfried Wilhelm Leibniz, and the compendious J. J. Moser. At their best, they rose above special pleading for a moribund political system and provided the materials for an intellectual defense of a federal state. The decline of Roman law as a self-evident source for all wisdom was followed by efforts to develop a specifically German constitutional law. The impact of Grotius caused the emergence of international law as a specialty within public law. In the eighteenth century, the doctrine of natural law supported a radical rethinking of legal structures in Cartesian terms. Stolleis remarks that the German search for a third way between despotism and revolution, one of the finest visions of German intellectual effort between 1750 and 1800, was only a step away from the nightmarish notion of a special German mission, which might even have to be imposed on an unwilling mankind.

Stolleis deals in a separate section with the emer-

gence of the discipline of *Policeywissenschaft*, which grew out of an encyclopedic tradition associated with *Hausvaterliteratur*, in which the prince or administrator sought to manage a harmonious estate that might be as large as an entire state. One price of this paternalistic approach was an almost complete elimination of independence on the part of the individuals whose happiness was the undertaking's ostensible aim. Immanuel Kant's trenchant critique of the concept of state-supported happiness undermined "*Gute Policey*" in the mid-eighteenth century, and as a result it evolved in the direction of administrative law.

Stolleis has provided a clearly written guide to a complex tradition, and his footnotes are virtually a purchase list of basic reading in early modern political and constitutional theory.

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NORBERT WASZEK. *The Scottish Enlightenment and Hegel's Account of 'Civil Society.'* (International Archives of the History of Ideas, number 120.) Boston: Kluwer Academic. 1988. Pp. xvii, 286. \$87.50.

The twentieth-century renaissance of interest in Hegel, fueled by Jean Wahl, Alexandre Kojève, Jean Hyppolite, and Herbert Marcuse, has tended to focus on the meaning and significance of Hegel's thought, his place in the German philosophical tradition, and his influence on Karl Marx. Norbert Waszek's purpose in this study is at once more modest and more novel: to investigate the much-neglected connection between the Scottish Enlightenment and Hegel's theory of "civil society." It is more modest because Waszek limits himself to establishing the influence of such major Scottish thinkers as David Hume, Adam Ferguson, Adam Smith, and Sir James Steuart on Hegel's views of the market economy, division of labor, poverty and its social consequences, the "right" ordering of society, education, and the "social good." It is more novel because what emerges from these pages is a "practical" Hegel, deeply concerned with topical issues and receptive to ideas outside of, and often hostile to, classical German philosophy.

In the first two chapters, Waszek provides a bibliographical documentation of the impact of the Enlightenment on Germany, making it the foundation of his analysis of the Scottish influence on the intellectual life of Germany and the framework for relating Hegel himself to the Scottish philosophers. This documentation shows that the Scottish thinkers enjoyed a wide and enthusiastic reception in eighteenth-century Germany, a reception evident in contemporary translations, reviews, and popularizations of their writings, as well as in the teaching of political economy at German universities, notably at Göttingen. Translators receive special mention, above all, Christian Garve (1742–98), who was read and admired by such eminences as

Johann Gottfried Herder, Johann Wolfgang von Goethe, Friedrich von Schiller, and Immanuel Kant and who did more than anyone else to spread the fame of the Scottish Enlightenment in Germany.

Waszek then proceeds to investigate Hegel's early manuscripts and other external evidence to document the dates and extent of Hegel's reading and other contacts with the Scottish Enlightenment. It was the social and political ideas of the Scottish thinkers that most interested Hegel, and those provide the basis of Waszek's discussion, in the concluding chapters, of Hegel's *Philosophy of Right* (1821), where the impact of those ideas is most evident.

Waszek shows how and to what extent this important treatise assimilates and reproduces the views of a variety of Scottish Enlightenment authors on such matters as needs, labor, exchange, social classes, interventionist qualifications to the idea of a free market economy, and division of labor. He also shows, in this context, that much scholarship on Hegel has operated with crude and inadequate conceptions of the Scottish thinkers, especially of Smith and Steuart. Regarding Hegel's discussion of social classes, for example, Waszek demonstrates that Steuart was a primary influence on Hegel, whereas, in Waszek's examination of Hegel's views on the division of labor, Smith and Ferguson are more relevant. In sum, Waszek makes a strong case for the view that Hegel's study and assimilation of the advanced economic theories of the Scottish Enlightenment thinkers enabled him to raise their conception of the modern market economy to the level of a comprehensive political philosophy and that Hegel's continued relevance is thus vindicated with respect to his account of "civil society."

Waszek's meticulously researched study does not purport to be a reinterpretation of Hegel or a systematic account of his philosophy. It does, however, shed light on a hitherto obscure but significant side of Hegel's development. Moreover, it presents findings and opens new directions for research, which should figure into any future intellectual biography or comprehensive assessment of a thinker whom Waszek calls "the philosopher of the modern world" (p. 230).

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HANS-ULRICH WEHLER. *Deutsche Gesellschaftsgeschichte*. Volume 1, *Vom Feudalismus des Alten Reiches bis zur Defensiven Modernisierung der Reformära 1700–1815*; volume 2, *Von der Reformära bis zur industriellen und politischen "Deutschen Doppelrevolution" 1815–1845/49*. Munich: C. H. Beck. 1987. Pp. x, 676; xi, 914. DM 136 the set.

These are the first two of a projected four-volume "societal history" of Germany from 1700 to 1945. Societal history, as Hans-Ulrich Wehler conceives it, is different from the garden-variety social history typi-

cally encountered in French or Anglo-American scholarship. No "history with the politics left out" is presented here; Wehler aspires to a genuinely total history organized around the dialectical interplay of three spheres: economics, power, and culture. Here the deliberate inclusion of politics reflects the fact that, for Wehler and others of his generation, the central problem of German history is political in character, namely, the origins and consequences of National Socialism. (On this point, see Hartmut Kaelble, "Sozialgeschichte in Frankreich und der Bundesrepublik: *Annales* gegen historische Sozialwissenschaften?" *Geschichte und Gesellschaft*, 13 [1987]: 78.)

In seeking to explain the events of 1933 and the roots of German "peculiarity," the work of Wehler and the so-called Bielefeld school has focused chiefly on the postunification era. This preoccupation with the period from Otto von Bismarck to Adolf Hitler also follows logically from Wehler's methodological agenda, with its aim of developing a historical social science. Because the categories and methods of social science in the West did not begin to be clearly formulated until the late eighteenth century, they are applied more easily to the modern period than they are to the social and economic structures of prerevolutionary Europe. The present volumes under review, then, are all the more interesting in that 1700, not 1871, is their point of departure.

Not surprisingly, however, the Kaiserreich casts its long shadow over Wehler's eighteenth century. Here we find various intimations of Germany's "separate path" (*Sonderweg*), including the constricting effects of East Elbian serfdom, the synthesis of bureaucratic absolutism with feudal social relations, the social and political ambivalence of the German middle classes, and the rise of Prussian militarism. Characteristically, Wehler writes of Prussian militarism that "the decisions made in this [the eighteenth] century were to have a fatal impact on the course of Germany history in 1807, 1871, and 1918" (vol. 1, p. 246).

Wehler's preoccupation with German "backwardness," of course, has drawn heavy fire from critics on the Left as well as on the Right. But in the context of the eighteenth century, whose dynamic features historians of Germany are apt to exaggerate, Wehler's emphasis on the persistence of Old Europe has its virtues. Underscoring the social, institutional, and fiscal constraints on territorial absolutism, he insists that eighteenth-century regimes were neither willing nor able to carry out the "revolution from above" with which they are sometimes credited. Fundamental change could only come from without, either as the direct consequence of French occupation, or through the "defensive modernization" forced on the regimes of the Napoleonic era. Even then Wehler, like Eckert Kehr, gives short shrift to the nationalist mythology surrounding the Stein-Hardenberg reforms. Wehler has some particularly harsh words for the "abstruse and reactionary" Freiherr vom Stein (vol. 1, p. 399), whose role and impact he believes to have been grossly

exaggerated. Wehler similarly disputes Wilhelm von Humboldt's role, arguing that the foundations of early nineteenth-century university reform had already been established at Göttingen in the eighteenth century. The Bavarian reformer Maximilian von Montgelas, on the other hand, fares much better here as a neglected figure whom Wehler considers the most successful statesman of the reform era.

In a similar vein, Wehler stresses that agrarian production and social relations remained preeminent up to 1848 and warns against exaggerating the importance of manufacturing and industry during the first half of the nineteenth century. The domestic market for industrial products remained small, factory output continued to be far less important than artisanal and protoindustrial production, and Germany's "great spurt" still lay ahead. But, although Wehler is highly sensitive to the forces of continuity that prevailed throughout the era, he does view the period from 1750 to 1850 as having constituted a fundamental break in German historical development. The changes he describes will be familiar enough to students of modernization theory: the transition from a corporate society of estates (*ständische Gesellschaft*) to a market-determined class society; the territorial consolidation of Germany in the wake of the Napoleonic invasions; the doubling of the population and agrarian production between 1815 and 1848; the triumph of agrarian capitalism and the dramatic growth of a rural proletariat, which was predominant by the eighteenth century; the rapid expansion of rural commodity production (although Wehler is skeptical about the existence of any direct link between protoindustry and the factory system); the transformation of the nobility into a class of capitalist producers; the gradual displacement of an older *Stadtbürgertum* by an entrepreneurial and professional bourgeoisie; and dramatic cultural mobilization marked by the expansion of schooling, literacy, and literary production.

The dislocations arising from that transition culminated in the "double revolution" of 1845–49, with Germany on the threshold of a successful industrial revolution and a failed political one. For Wehler, the revolution of 1848 was at heart neither a belated reaction to the economic crisis of 1846–47 nor a class struggle but the outgrowth of a series of "modernization crises" that shattered the legitimacy of the political, economic, and cultural inheritance of Old Europe. Although the events of 1848 remain for Wehler a failed revolution, he attributes that failure less to the weakness of the revolutionaries than to the strength of their opponents. He also emphasizes that the revolution was not without its achievements, above all the eradication of feudalism in the countryside.

Comprising with footnotes more than fifteen hundred pages, these volumes are a testimony to Wehler's enormous energy and productivity. Whether hailing or lamenting his indisputable impact on the writing of German history, few will fail to be impressed by the breadth of his reading or the stupendous amount of

empirical evidence he has assembled and synthesized. The two volumes contain forty-six tables with useful statistical data on subjects such as population, infant mortality, grain prices, tax revenues, railroad construction, coal production, the social composition of town and countryside, the production of newspapers and books, elementary school attendance, and university matriculation.

As with much of his work, however, Wehler's methodological asides and theoretical prolegomena can easily drive the reader to distraction. It is as if Wehler feels compelled to reinvent the wheel at each turn, embarking on lengthy and often familiar excursions into the rise and development of feudalism in the Middle Ages, the origins of the American and French revolutions, or the rise of capitalism in Western Europe. His intentions, to be sure, are laudable. Conceiving of the historical enterprise much as Jürgen Habermas does, Wehler insists that historians must be theoretically explicit if they are to engage in rational, critical discourse with each other and with their audience. In practice, however, the effect is frequently the opposite. Despite the long and lavish footnotes, Wehler's citations are spaced too far apart to allow the reader to pin down the exact sources of a particular assertion. Moreover, the authorities he cites are a disparate crew—Karl Marx and Max Weber, Rolf Dahrendorf and Nicos Poulantzas, Maurice Dobb and W. W. Rostow, Alexander Gershenkron and Eric Hobsbawm, to name just a few—and it is not always clear where his agreement with one begins and ends.

Although Wehler's eclecticism sometimes obscures the methodological transparency to which he aspires, two things emerge clearly from these volumes. The first is his overwhelming debt to Max Weber, whose theory of modernization Wehler deems far superior to that of Marx (see vol. 2, p. 600). The second is implicit in his relatively positive assessment of the events of 1848, which suggests that he has further distanced himself from the model of German divergence expounded in his earlier work. Even more explicit in this regard is Wehler's admission, buried in the footnotes at the end of the second volume, that "industrialization and democratization do not necessarily go hand in hand" and that "industrial modernization is thoroughly compatible with authoritarian political structures" (vol. 2, p. 878).

Given these concessions, one might well ask what is left of the "Bielefeld paradigm." The next two volumes will doubtlessly provide an answer, but, whatever emerges, Wehler's work is sure to be praised, criticized, and consulted for years to come.

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BERND VON MÜNCHOW-POHL. *Zwischen Reform und Krieg: Untersuchungen zur Bewusstseinslage in Preussen 1809–1812.* (Veröffentlichungen des Max-Planck-Instituts

für Geschichte, number 87.) Göttingen: Vandenhoeck und Ruprecht. 1987. Pp. 479. DM 98.

Bernd von Münchow-Pohl's objective is to describe the "consciousness" of Prussia during the traumatic years between the fall of Karl vom Stein and the Wars of Liberation. He promises a new way of looking at an epoch that historians have analyzed frequently and from a variety of perspectives.

The author describes Prussians' reactions to a succession of bewildering experiences thrust on them: shocking military defeat, foreign occupation, physical damages of war, spiraling inflation, disastrous harvests, and constant uncertainty caused by socioeconomic reforms. Through extensive research, using an impressive array of archival and published sources, he portrays the views of a wide spectrum of the population, including entrepreneurs, peasants, estate owners, military leaders, and educated civil servants. Predictably, Prussia did not speak with a uniform voice. Responses ranged from the "naive royalism" (pp. 61, 207) of peasants to the alarm of the middle class over property devaluation to the allegiance of some military leaders to a cause higher than their vacillating government. The years 1809–12 were at once a time of confusion, mixed loyalties, ferment, hope, and despair. A sense of crisis was pervasive, and Münchow-Pohl depicts concretely that widely shared mood.

Whether the author succeeds in portraying the "subjective . . . consciousness" of Prussia as he planned, however, is questionable (p. 13). One confusing factor is the large proportion of the text that he devotes to governmental leaders. It is not clear whether that analysis is intended to provide a context for his study of consciousness or whether he considers the bureaucracy to be a prime facet of the Prussian consciousness. Whichever the case, the author employs a variety of superficial characterizations of the reformers, arguing, for example, that a "lack of will and conceptualization" (p. 64) among Stein's colleagues hindered their work and that the situation worsened after Stein's dismissal because his successors were deficient in "leadership qualities" (p. 185). Such claims reveal little that is new about the reform movement. Moreover, if Münchow-Pohl intended to present an analysis of political leaders and their work, he should have consulted the recent scholarship more thoroughly, including the innovative research of several historians of the German Democratic Republic: Helmut Bleiber, Hartmut Harnish, Hans-Heinrich Müller, Rudolf Berthold, and Heinrich Scheel. It is surprising to find few East German publications in the bibliography.

Münchow-Pohl's useful study would contribute more successfully to historical scholarship if the author had defined precisely what he meant by both words in his phrase "Prussian consciousness." Were Chancellor Karl August von Hardenberg and Queen Louise, both of whom are objects of analysis in this book, part of the Prussia Münchow-Pohl describes, or were they figures to whom Prussia was reacting? Did consciousness in-

clude reform policies, military planning, and diplomacy, or were those among the factors that spawned a consciousness?

Although Münchow-Pohl raises more questions than he answers, historians will nevertheless find his work very useful. His concentration on the spectrum of responses to Prussia's crisis adds a new dimension to the historical literature on the crucial transition in the Hohenzollern lands during the Napoleonic era.

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DANIEL J. HUGHES. *The King's Finest: A Social and Bureaucratic Profile of Prussia's General Officers, 1871–1914*. New York: Praeger. 1987. Pp. ix, 215. \$42.95.

Although there have been some mildly quixotic attacks on the thesis, it is generally accepted that the aristocracy played a disproportionately important role in the Prussian officer corps, particularly in the higher ranks. Daniel J. Hughes is untiring in his admonitions to earlier historians who have had the temerity to point out the aristocracy's role, handing out bad marks for lack of originality, yet he devotes an entire book to making exactly the same point. Hughes's book is, of course, a major departure from earlier naive attempts in that it is a "prosopographical study," the data bunched into a computer and cross-tabulated by means of a tricky piece of software with the curiously military-sounding acronym SAS. The result is that we now know what we knew already, but at least we know it to two points of decimals. This study is, in short, "scientific."

Most generals came from East Elbian aristocratic, Protestant families. Almost half of the officers promoted to brigadier general came from the *Uradel*—families whose titles dated from before 1350 (not 1400 as Hughes claims). Aristocrats advanced more rapidly through the ranks than those officers of bourgeois background and had a far better chance to reach the top. One interesting point emerges from the analysis of the biographical data of 2,443 generals. Attendance at the war academy was not an essential precondition for a successful military career, as I for one had assumed. Most generals apparently never went near the place.

In a study that sets such a high premium on precision and percentages, it is curious that the aristocracy is divided into two simple categories of *Uradel* and *Briefadel*. That categorization takes no account of rank and groups a simple "von" with a duke. Nor does it make the important distinction between mediated and unmediated aristocracy. Furthermore, an aristocratic family, however ancient, that made a misalliance was immediately demoted in or removed from the *Almanach de Gotha*, in which the European aristocracy was listed, and ceased to be highly regarded.

A central thesis of this book is that being in the service of the state was more important to a successful military career than the ownership of land. That thesis

is based on an unsatisfactory definition of landownership. Hughes states that the father of one general in four was "of the rustic type" and that, of those, one in three was a working farmer. That statement is not very helpful. Does it mean that three-quarters of the generals owned no land at all? Or is Hughes talking about aristocratic estates (*Rittergüter*)? In part such uncertainty arises from confusion over the definition of the term *Junker*, which admittedly is hard to pin down satisfactorily. Similarly, the assertion that 43.37 percent of generals had no private income is very hard to believe, particularly because nearly all of them had such support when they embarked on their military careers.

The study suffers from two additional, serious weaknesses—a reliance on *post hoc* argument and a fondness for straw men. The social background of an officer is not a complete and satisfactory explanation of the reason for his promotion. Everyone knows that Alfred von Schlieffen was an appalling snob, and the general staff was certainly not free of social prejudice. There is no point in endlessly repeating that. But von Schlieffen was also aware that the size of the army had to be increased if his famous plan were to have a chance of success. A larger army meant that some of the socially undesirable would have to become officers. That was a central dilemma in the Prussian army, but it is not revealed by vulgar empiricism.

Rich bourgeois ("wealthy peasants" [p. 47]) did not serve in the ranks. They either became reserve officers or did not serve at all. General staff officers, unlike the officers in the Eleventh Hussars, did not wear red trousers. The officer corps did not "expire" (p. 151) in November 1918. This generally tiresome book is, however, rendered readable by unintentional humor. Bourgeois officers, we are solemnly told, were more likely to come from business families than were aristocrats. Young men with large private incomes apparently had a more comfortable time in the army than did those without such incomes. Infantry officers, unlike the cavalry, were not required to buy horses.

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SIEGBERT WOLF. *Liberalismus in Frankfurt am Main: Vom Ende der Freien Stadt bis zum Ersten Weltkrieg (1866–1914)*. (Studien zur Frankfurter Geschichte, number 23.) Frankfurt, F.R.G.: Waldemar Kramer. 1987. Pp. 268.

"Like an oasis in the wasteland" (p. 5) was how Leopold Sonnemann, the most important Frankfurt liberal in the Second Reich, described his native city in the sociopolitical landscape of imperial Germany. It was not always so.

This first-rate study begins when the Free City of Frankfurt was annexed by Prussia and reduced to the status of a provincial capital. Within only three years of Frankfurt's "degradation," an economic upswing be-

gan that transformed this tradition-rich metropole of trade into a modern industrial city. The city's leading economic elites accommodated themselves to this process with surprising ease, seeing their profits rise and their social status confirmed in the changed circumstances of the empire. Economic and social conformity, however, was not equally attractive to all liberals; by the very first years of the Kaiserreich important fissures were appearing on the Left. The years from 1868 to 1873 merit the very detailed coverage they receive here, because in these years the local party system was evolving rapidly without yet re-creating political divisions found at the national level. Eventually, though, the topography of Frankfurt's liberal movement was not very different from the political landscape in Berlin, except to the extent that the persistent "politics of notables" was bolstered locally by one of the most restrictive municipal franchises in Germany.

The main chapters of this book detail the organizational evolution of the local parties, their electoral fortunes, and their work in the city assembly. Separate chapters are devoted to franchise reform, to the incorporation of outlying suburbs, and to the campaign for a university. There is careful attention throughout to the unique role of the liberal press in Frankfurt and to the social constituency of the various groupings on the Left. The author rightly draws attention to the increasing tendency of most Frankfurt liberals to deny class conflict and complacently support the political status quo, especially after 1900. As Wolf observes, "Relatively secure on the foundation of the Wilhelmine monarchical status quo, the left-liberal burghers found sufficient opportunities to put into practice their economic, social, and cultural policies" (p. 73).

In this story of the accommodation of Frankfurt liberals to an illiberal political culture outside their city walls, the Red threat, of course, played an important role. The author's excursus on social democracy, however, fails to address this problem sufficiently, and the presentation and accuracy of a number of statistical tables could have been improved (the penultimate line of the table on page 175, for instance, shows that by 1912 fully two-thirds, not one-third to one-half, of Frankfurt's Reichstag voters also had the municipal franchise). These shortcomings are compounded by a lack of detail on such frequently mentioned groups as the Center party, the Conservative Union, the Homeowners Association, and the Frankfurt Mittelstand Association. By focusing too narrowly, the author misses tantalizing opportunities to extend his analysis. A good example is his failure to comment on the implications of Frankfurt's National Liberals agitating in favor of a university, while the National Liberal leadership in Berlin was simultaneously blocking implementation of the necessary legislation in the Prussian Landtag. Similarly, the local context of franchise reform agitation in 1910 is addressed too fully and the national context too fleetingly to make the analysis as rich as it could be. Perhaps Wolf was too intent on substantiating his introductory remark (repeated three times) that Frank-

furt was not typical of even the larger German cities of the day.

There is remarkably little else to find fault with in this book. In part because the work touches on the intersection of so many neglected areas of research—left liberalism, municipal politics, franchise reform, and the consolidation of “national” politics in the years from 1866 to 1890—it is an important and welcome addition to the literature on the Second Reich.

JAMES RETALLACK
University of Toronto

HELMUTH TRISCHLER. *Steiger im deutschen Bergbau: Zur Sozialgeschichte der technischen Angestellten 1815–1945*. (Bergbau und Bergarbeit.) Munich: C. H. Beck. 1988. Pp. 490. DM 42.

Of all German industries, mining has probably attracted the most attention from historians. The geographic concentration of mining's sizable work force, the wide-ranging influence exercised by its entrepreneurial elite, and the drama and political significance of miners' strikes go far in explaining the industry's attractiveness as a topic for investigation. So does the availability of comparatively abundant sources in state and private archives. Yet significant gaps remain in the literature. Helmuth Trischler has addressed one of the most important of those gaps with his carefully researched study of supervisory personnel (*Steiger*) in the mines.

Given the dispersed nature of underground toil, effective oversight of miners at work was especially difficult to achieve. Wages based on piece rates, combined in the twentieth century with increased mechanization of production, represented partial solutions to the discipline problems employers faced. But mine operators continued to regard the determined efforts of loyal supervisors as crucial and went to great lengths to make certain that such efforts would be forthcoming.

Trischler ambitiously chooses to cover almost a century and a half of mining history, from the days of state direction during the early nineteenth century to the fall of the Third Reich. He recounts changes in the recruitment, training, and remuneration of *Steiger* over that period. In addition, he traces the development of organizations that sought to articulate supervisors' interests. His account is particularly detailed for the crisis years immediately following World War I, a time of uncertain loyalties when mine supervisors struggled to redefine their relationships with organized labor and with their employers.

A number of themes recur in Trischler's study, especially those relating to the position of the *Steiger* as men caught in the middle. As vital intermediaries between employers and miners, supervisors faced the difficult task of enlisting the cooperation of their subordinates while assuring their superiors that company dictates were being implemented without qualification.

Steiger also found themselves suspended between their employers and the state. The mine operators expected them to give priority to achieving production that was cheap and reliable. To that end, production premiums constituted a major component of *Steiger* income. The state, for its part, assigned major responsibility to the *Steiger* for making certain that safety regulations were observed, even if the consequence was reduced output and therefore less pay for supervisory personnel.

Trischler gives particular attention to the role of mining schools in the training of supervisors. He relates his discussion of the selection of students to questions of advancement opportunities for miners. He stresses the importance of arduous years of combined study and shift work in shaping the attitudes of future *Steiger*. And he details the employers' struggle to maintain their control over the schools and their students, a struggle that acquired particular significance during the National Socialist years.

Chapter by chapter, Trischler offers brief summaries of studies on salaried employees in other industries and notes whether he finds mining supervisors conforming to the models suggested. He is, however, cautious about making generalized pronouncements of his own. He limits himself largely to the investigation of job-related developments and appends a mere handful of pages hastily sketching the lives of supervisors above ground. He offers few international comparisons, and those he does cite are hidden away in footnotes. Readers not already familiar with the history of German mining may regret Trischler's failure to identify adequately the institutions and individuals he discusses. But on balance Trischler has produced a very useful book, offering an abundance of information drawn from rich documentary sources about an important and previously neglected group.

ELAINE GLOVKA SPENCER
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JERRY Z. MULLER. *The Other God That Failed: Hans Freyer and the Deradicalization of German Conservatism*. Princeton: Princeton University Press. 1987. Pp. xv, 449. Cloth \$49.95, paper \$14.95.

Once prominent among Weimar conservative revolutionaries, the Leipzig sociologist Hans Freyer was relegated to obscurity by postwar German intellectual politics. Jerry Z. Muller has attempted not only to renew interest in Freyer but to write a “representative biography” (pp. 4–9) examining him as a figure exemplifying general patterns in German radical conservative ideas and experiences during the twentieth century. Emphasizing the contentious development of German social science, particularly vis-à-vis ideology, Muller traces that intellectual movement from its early anti-individualistic critique of modern society, through its initially hopeful but eventually disillusioning compromises with Nazism, to its post-1945 acceptance of pluralistic society, liberal democracy, and the capitalist

welfare state. Ultimately, a universalistic, humanistic perspective on life and a broader European concept of history and culture replaced the narrow *völkisch* nationalism that had not only failed but also proven so destructive.

Considering his book a "case study in the relationship of intellectuals to totalitarianism" (p. 24), Muller argues that before Nazism emerged as a force, Freyer consistently sought a totalitarian solution to the problems of modernity, since a predilection for an "all-encompassing" *Weltanschauung* (p. 27) remained an inescapable legacy of his early religiosity and intellectual milieu. Orthodox theology gave way to an equally comprehensive political ideology that would fill the void left by philistine, secularized modern life with a renewed communal sense of meaning and purpose. A historicist who challenged the Enlightenment claims of universal reason, humanity, and scientific objectivity, Freyer devoted himself as a Weimar ideologue and academic sociologist to development of a subjectivistic, *völkisch*-oriented social science that demanded engagement to create a "totally integrated society" with common beliefs, values, and goals (p. 11). Freyer, Muller asserts, endorsed Nazism as early as 1931, primarily because it offered the possibility of realizing those objectives. Telling the story of the deradicalization of both Freyer's social theory and German conservatism generally that followed disillusionment with Nazism is among this book's most valuable contributions.

Yet it is precisely regarding the subject of totalitarianism that Muller's study is vulnerable to serious criticism. Muller's conceptualization and explanations of totalitarianism, especially as they pertain to affinities and distinctiveness in conservative and Nazi ideologies, are narrow, one-sided, or misleading. Abandoning the meticulous, accurate, and balanced analyses characteristic of his examination of Freyer's peculiar adaptation of the ideas of more highly regarded thinkers (for example, Georg Simmel), Muller presents a simplistic, distorted rendition of conservative thought on the "total state" (pp. 210–16). Ignoring the diversity and complexity of conservative revolutionary ideas, Muller holds that radical conservatives generally had totalitarian goals, when actually Freyer's predisposition for such totality was more the exception. One could debate, in fact, the entire section on Freyer's *Revolution von rechts*. Otherwise, Muller's scholarly detachment toward his controversial subject, like his rare sensitivity in understanding the conservative cultural critique of modernity, is particularly noteworthy.

Generally, Muller provides compelling arguments based on sound interpretations of extensive archival sources and a wide range of secondary literature. And despite problems of redundancy and countless rhetorical questions, he synthesizes incisive analyses of the theoretical, academic, and political dimensions of German sociology and conservatism into a well-written, lucid, and intellectually stimulating historical narrative. This book is an impressive achievement

that ranks among the very best studies of German conservatism.

JOSEPH W. BENDERSKY
Virginia Commonwealth University

PETER MANSTEIN. *Die Mitglieder und Wähler der NSDAP 1919–1933: Untersuchungen zu ihrer schichtmässigen Zusammensetzung*. (Europäische Hochschulschriften, Series 3, Geschichte und ihre Hilfswissenschaften, number 344.) New York: Peter Lang. 1988. Pp. 233. \$46.15.

Happily, historians investigating the social basis of National Socialism largely seem to agree. Answers to questions such as who voted for Adolf Hitler and who joined Hitler's party are pretty clear. Supporters did not just come from grim and impoverished middle-class backgrounds; substantial numbers were blue-collar workers and well-to-do professionals and business owners. Moreover, not all middle-class constituencies voted for the Nazis in large numbers. Historians have come to pay closer attention to fine and politically consequential differences in status and occupation. The resulting picture of the Nazi membership and electorate is far more socially diverse than has been traditionally believed. With its broad and differentiated base of support, Hitler's National Socialist German Workers' Party was Germany's first *Volkspartei*.

Peter Manstein takes stock of the recent literature on Nazism's social base. His monograph does the very useful service of reviewing the statistical and quantitative inventory. Manstein points out untested assertions and misleading controls and locates arithmetic errors and nonrandom samplings. A careful comparison of investigations of the Nazi membership, analyses undertaken recently by Michael Kater and James Madden and, in the early 1930s, by Werner Studentkowsky, is the most valuable part of the book. A briefer examination probes the electoral studies of Thomas Childers, Jürgen Falter, and others. But, in the end, for all the commotion, Manstein provides little that is new. Given the difficulties involved in preparing a representative slice of the membership and identifying accurately social strata and occupation, the data that Manstein reevaluates are remarkably consistent.

Manstein reasonably calls for even more detailed studies. He shows, for example, that an overrepresentation of skilled workers among Nazi members is hidden by the underrepresentation of workers generally. In this regard, Manstein takes historians to task for assuming rather than analyzing the motives of workers and for investigating only industrial workers. Indeed, one area that needs to be researched is the affinity between German workers and National Socialism. By the same token, employees did not march lockstep into the Nazi camp. Strongly pro-union and sympathetic to the Social Democrats, many white-collar clerks wanted nothing to do with Hitler. Others voted Nazi only in periods of crisis. Much more attention should be paid to these subgroups, though Manstein overstates exactly

how crude historians have been until now. Nobody will argue with Manstein's admonition that scholars studying voter motivations should consider the fact that constituents often heard the Nazis say things with which they disagreed, even if the Nazis broadcast what they believed voters wanted to hear. The point is uncomplicated but worth repeating.

If historians know a great deal about who voted for Hitler, they need to know much more about why particular social groups did so. Unfortunately, Manstein's review of the literature examining the motives of voters and joiners is extremely cursory and ultimately unhelpful. Too often Manstein refuses to draw conclusions and thus leaves the reader dangling. Although he provides valuable insights and locates a half-dozen dissertation topics, he does not bring cohesion to his enterprise. Manstein's laudable contribution is to evaluate statistical analyses of Nazi support, but even here the effort is marred unnecessarily by an appendix full of welcome but sloppily presented tables.

PETER FRITZSCHE
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HENRICH AUGUST WINKLER. *Der Weg in die Katastrophe: Arbeiter und Arbeiterbewegung in der Weimarer Republik 1930 bis 1933.* (Geschichte der Arbeiter und der Arbeiterbewegung in Deutschland seit dem Ende des 18. Jahrhunderts.) Berlin: J. H. W. Dietz. 1987. Pp. 1025. DM 118.

The third volume of Heinrich August Winkler's massive trilogy on the history of workers and the labor movement in the Weimar Republic opens with a one-hundred-page survey of unemployment, the problem that did so much to shape working-class experience in the years 1930–33. Mass unemployment led to political apathy; deepened divisions between the employed, who tended to stick with the Social Democrats, and the jobless, who gravitated toward the Communists; and robbed workers of the political instruments, such as the general strike, with which they had defeated the Kapp Putsch in 1920. The impotence of the labor movement was compounded by the spread of mass culture, which Winkler argues was an indicator not so much of *embourgeoisement*, as contemporary sociologists maintained, as of “deproletarianization,” an undermining of the class consciousness that bound workers to socialism. Although those arguments are only sketched in, they are persuasive and set an effective backdrop for the political narrative that occupies most of the rest of the volume.

For Winkler, the central problem of those years was the Social Democrats' toleration of government even at the price of swallowing increasingly undemocratic and deflationary policies. This toleration might have been justifiable as a means of dissuading Heinrich Brüning's ministry from interfering with social democratic control over Prussia, but it was hardly defensible once

Brüning was gone. The party's pathetic decision to back Paul von Hindenburg for president in 1932 was justified by Prussian minister-president Otto Braun in terms of such enthusiasm for the aged monarchist field-marshal that one is left with no very favorable impression of the party's sense of political realism at this stage. When Braun's government was illegally removed by what was in effect a military *coup d'état* in July 1932, the possibilities of successful resistance, whether by legal action, general strike, or armed struggle, were, argues Winkler, very limited. Even so, he concedes, the Social Democrats did so little to prepare for the event, despite knowing about it weeks in advance, that their removal from office was “more a farce than a tragedy” (p. 679). Still worse was to come in early 1933 when the trade unions detached themselves from the socialists and tried to save themselves by cooperating with Hitler, while the Social Democratic deputies covered themselves in shame, however defensible their motives, by voting for the Hitler government's disarmament declaration in the Reichstag on May 17. Only Otto Wels's moving final speech in the chamber in March provided a glimmer of light in the enveloping gloom. As for the Communists, Winkler makes clear once more his view that their policy of outright hostility to the Social Democrats contributed significantly to the republic's downfall.

Winkler tells this miserable story with the same stylistic clarity, intellectual openness, and scholarly thoroughness that distinguished the first two volumes of his trilogy. The three volumes taken together, covering some twenty-seven hundred pages in all and weighing in at just under ten pounds, constitute a staggering achievement, literally so if one attempts to carry all three volumes in one's briefcase at the same time. This will be a standard work for years to come. Yet it is not without its faults, and these weigh particularly heavy in the final volume. Winkler subordinates everything to the question of how the Social Democrats could have saved Weimar democracy, so that far too much space is allotted to a narrative of their politicking in Berlin. As their actions became progressively more irrelevant to the real course of events, so Winkler devotes more and more space to analyzing theoretical articles by socialist intellectuals observing the scene from a distance. Moreover, the individual actors in the drama are never given any real human identities; they remain just names, with the single exception of Hermann Müller, the real hero of the trilogy, who is the subject of an uncharacteristically emotional obituary (pp. 295–97). New figures continually appear in the leadership without one being told who they were, how they got there, or what their relations were with one another. In what is for very long stretches a political narrative of the most traditional kind, the lack of a human dimension, so characteristic of the writings of the generation of structural historians to which Winkler belongs, becomes a real disadvantage and detracts seriously from the book's otherwise outstanding readability.

Winkler's concentration on the Reichstag and government means an unfortunate neglect of many other aspects of labor movement history, from the performance of social democratic administrations at every level—from the Prussian government down to the municipalities—to the party's attitude to issues such as policing and judicial reform. Winkler is even less informative about the Communists in these respects than he is about the socialists. He has little to say about the theoretical debates that took place within the movement and neglects party organization, the party press, and many other subjects. His concern to defend the Social Democrats' actions in the revolution in volume 1 undermines the attempt to explain their failures and compromises in volume 3. Above all, these volumes cannot be regarded as the definitive text on the social history of the working class, which occupies only about 15 percent of the work as a whole. The final volume is particularly sketchy, with much of the space being taken up by an account of unemployment in Austria and a discussion of various theoretical writings by sociologists. Winkler, therefore, has given us a splendid account of the history of the German labor movement under Weimar, but the history of the German working class in this period remains to be written.

RICHARD J. EVANS
University of London

PAUL KREMMEL. *Pfarrer und Gemeinden im evangelischen Kirchenkampf in Bayern bis 1939: Mit besonderer Berücksichtigung der Ereignisse im Bereich des Bezirksamts Weissenburg in Bayern.* (Studien zur neueren Kirchengeschichte, number 1.) Lichtenfels, F.R.G.: H.O. Schulze. 1987. Pp. v, 784. DM 39.

Local and regional studies of the church struggle continue to multiply apace. Some are of little more than antiquarian interest, but the best make solid contributions to continuing scholarly dialogues over popular culture, social structure, and the exercise of power in Nazi Germany. To some extent, Paul Kremmel's dissertation on the church struggle in Middle Franconia may represent an example of this "new local history," but, if so, it can hardly be said to have realized its full potential.

The book's basic strategy is promising and well conceived. His aim, Kremmel states, is "to write the early history of the Church Struggle in Bavaria 'from below,' as [Protestant] pastors and congregations experienced it" (p. 2). For this purpose the area of Franconia around Weissenburg seems an ideal choice of focus. A region of small towns and rural villages south and west of Nuremberg, lying astride Bavaria's historic confessional frontier, it was marked during the Hitler era by an unusual conjunction of rabid National Socialism—this was Julius Streicher's home turf—and levels of popular religiosity far above the average for Protestant Germany. After sketching social and ecclesiastical conditions in Weissenburg, Kremmel details the disin-

tegration of early euphoric alliances between pastors and party members, traditional nationalists and *völkisch* nationalists. In the process he documents the often-erratic course of Nazi church policy and the profoundly ambiguous stance of the "intact" Bavarian church, which found most pastors warmly affirming the Nazi state and its purposes, while at the same time seeking to disavow core elements of Nazi ideology. Although the basic story is a familiar one, Kremmel's account is replete with revealing vignettes and interesting minor characters. The American-born author provides an intimate and convincing sense of the Bavarian milieu.

Even so, this is not a particularly satisfying book. The root problem is Kremmel's failure to propose any clearly articulated thesis or heuristic framework. His approach is almost purely descriptive; nowhere does he explicitly identify or address general issues of interpretation suggested by his research. The research itself, to be sure, is wide-ranging, systematic, and, as with many dissertations, thorough to a fault. No relevant source appears to have escaped Kremmel's attention, and almost none goes uncited (one chapter boasts no fewer than 1,291 footnotes). All too often, however, reportage is of the quote-and-summarize variety; Kremmel compiles a surfeit of information but seldom uses it to fashion a sustained argument. Because the narrative tends to follow where abundance of source material leads, the focus shifts frequently; Weissenburg, for example, fades from view for long stretches (although the sections dealing with it are among the book's most interesting). And, while Kremmel needs two hundred pages of text to relate the many well-known confrontations and controversies of 1934, he disposes of the entire period 1936–39 in less than twenty, even though it is just his sort of detailed local research that might conceivably have brought new insights to bear on these years of routine and growing resignation.

In sum, Kremmel's study begs for a more productive balance between information and interpretation. Its wealth of raw material nevertheless gives it considerable reference value, which makes the absence of an index as lamentable as it is astonishing.

DAVID J. DIEPHOUSE
Calvin College

BERND RÜTHERS. *Entartetes Recht: Rechtslehren und Kronjuristen im Dritten Reich.* Munich: C. H. Beck. 1988. Pp. 226.

German historical scholarship often reveals more about current ideological conflicts than it does about the past. And Bernd Rüthers's book, which makes the indisputable point that legal theory and practice in the Third Reich degenerated into an extraordinary perversion of both law and justice, is no exception. For the work of this law professor and judge is significantly affected by his concerns about certain current tendencies in German legal thought, as well as by the renewed interest in

Carl Schmitt among conservative and leftist intellectuals.

The first thirty pages offer an excellent introduction to the transformation of legal thought after 1933, which explains how "antinormativism" (pp. 20–27), like new concepts and sources of law, was directed toward a *völkisch* rejuvenation (perversion) of law under Nazi ideological hegemony. But, thereafter, the book becomes almost entirely a work on the controversial jurist Carl Schmitt and fails to discuss most of the legal theory, practice, and institutions in the Third Reich. While concentrating on Schmitt, a figurehead whom the Nazis rejected by 1936 precisely because his theories lacked racial and anti-Semitic foundations, Rüthers gives the leading advocates of an inherently racist legal theory (for example, Otto Koellreutter and Reinhard Höhn) mere passing references. Disturbed by a renaissance in "concrete order thought" in the form of "institutional legal concepts" in West Germany, Rüthers does analyze Karl Larenz's synthesis of Hegel, "concrete-general-concepts" of law, and Nazi racial ideology (pp. 57–98). But Rüthers's untenable interpretation that Schmitt's theory of "concrete orders" was also racial is based on references and quotations from Larenz rather than from Schmitt. For an undistorted explication of this legal concept as propounded by Schmitt, one should read the works of Ernst-Wolfgang Böckenförde, a Freiburg law professor and member of the constitutional court of West Germany.

Rüthers considers Schmitt a paradigm for German jurists under Adolf Hitler's dictatorship. In a polemical survey of Schmitt's major publications and other contentious aspects of his public life during this period, Rüthers revives an old interpretation of Schmitt as an underminer of Weimar democracy who enthusiastically embraced Nazism. Not only is it questionable whether the enigmatic Schmitt, despite his notoriety and significant influence, is sufficiently representative to be used as a paradigm, but this particular interpretation has already been refuted by the available evidence and recent historiography. Rüthers is aware of the evidence and the scholarship, yet he either ignores or attempts unsuccessfully to argue both away. Schmitt is, of course, a figure justifiably open to severe criticism; certainly no excuses are possible for his reprehensible compromises with Nazism. Rüthers, however, provides no analysis of Schmitt's works during the Weimar period, and, when discussing Schmitt's post-1933 writings, Rüthers takes quotations out of context or fails to consider the complex, changing political and personal circumstances in which such works were written. Most revealing, though, is that Rüthers would take seriously the absurd thesis of Nicolaus Sombart (one never actually fully developed let alone substantiated psychoanalytically or otherwise by either scholar) that Schmitt's theories and behavior emanated from his attempt to overcome his own bisexuality by suppressing everything feminine. Supposedly, Schmitt's concepts were a projection of his militant, masculine power

fantasies and obsession with patriarchal society, which included a fetish for the state.

Rüthers's real contribution is an incisive concluding section on the "lessons" derived from studying the perversion of law under Nazism in which he argues convincingly that jurists can learn as much from the transformations following 1933 as they can from the turning points of 1918–19 and 1945–49. Unfortunately, these themes do not constitute the core of his study. Essentially, this is a book for those well versed in German legal theory or intimately familiar with the historiographical intricacies and polemics of the debate over Schmitt.

JOSEPH W. BENDERSKY
Virginia Commonwealth University

AVRAHAM BARKAI. *Vom Boykott zur "Entjudung": Der wirtschaftliche Existenzkampf der Juden im Dritten Reich 1933–1943*. Frankfurt, F.R.G.: Fischer Taschenbuch. 1988. Pp. 234. DM 14.80.

This book is the first scholarly examination of the impact of Nazi economic policies on the German Jews. Avraham Barkai makes a strong case for the view that the Jews were more profoundly affected by the slow erosion of their income between 1933 and 1938 than by such dramatic events as the Nuremberg Laws of 1935 and the *Kristallnacht* pogroms three years later. By early 1938, Barkai argues, most of Germany's surviving Jewish population was unemployed, its businesses "Aryanized," and its residual economic life ghettoized. Barkai's most valuable contribution is in showing how those situations resulted from illegal but officially tolerated actions that often varied from place to place: unofficial local boycotts, arbitrary denials of public social services, intimidation to persuade Jews to sell their property, and pressure from Nazi unions to fire Jewish employees. And yet important reservoirs of Jewish wealth survived both the structural changes of the Weimar years and the exclusionary pressures of the Nazis. This wealth helped maintain an impressive network of social services and charitable institutions that eased the Jews' burdens and—sadly, in retrospect—removed some of the urgency from emigration. Barkai does not, however, explore the conflicts that existed within German Jewry about how best to respond to Nazi policies, giving the misleading impression that pre-1938 Jewish opinion was, for the most part, static and uniform.

Unlike many who have written about the Jews in Nazi Germany, Barkai pursues his subject beyond the outbreak of war and the first deportations to Eastern Europe. He briefly sketches the imposition of forced labor in Germany and takes a position on the disputed role of the last officially recognized national Jewish organization, the Reichsvereinigung (Reich Union). It could not help being a tool of the Gestapo, he convincingly argues, for only in that way could the union continue to serve the needs of those dependent on it.

As was true of the Jewish Councils in the ghettos of Eastern Europe, there was no practical means of escaping complicity in the crimes of the Nazis.

In what is undoubtedly the most controversial and probably the most vulnerable aspect of his study, Barkai attacks the widely held view that early Nazi anti-Semitic policies were haphazard products of struggles between rival party and state institutions. Rather, he insists, they were deliberately planned and conscientiously pursued by the party with full cooperation from the government and the private sector. The nationwide boycott of April 1, 1933, often portrayed as an improvised sop to National Socialist radicals, is for Barkai a deliberate first step in the systematic and increasingly radical exclusion of the Jews from the German economy. The author's indictment of government officials extends to Hjalmar Schacht, who is exposed as an ineffectual opponent of a few of the most extreme Nazi policies and as no friend of the Jews. The verdict on Schacht apart, Barkai's arguments are not as well documented as those in studies by Uwe Adam, Helmut Genschel, and Karl Schleunes, all of which reach conclusions opposed to his. Future researchers will keep Barkai's doubts in mind, but for the moment there is no reason to abandon the impressive structural analyses of the scholars named above.

DONALD L. NIEWYK
Southern Methodist University

HANS-WALTER SCHMUHL. *Rassenhygiene, Nationalsozialismus, Euthanasie: Von der Verhütung zur Vernichtung 'lebensunwerten Lebens,' 1890-1945.* (Kritische Studien zur Geschichtswissenschaft, number 75.) Göttingen: Vandenhoeck und Ruprecht. 1987. Pp. 526. DM 88.

This study details the genesis and development of racial hygiene in the German context and explains its implementation in the form of, first, sterilization and, then, what the Nazis euphemistically called "euthanasia." In the first half of the volume, Hans-Walter Schmuhl deals with theoretical underpinnings; in the second he exposes Nazi actions in the infamous killing machine, stopping short of the gassings of Jews in the liquidation camps. (This last-mentioned consequence is presently seen by several historians as the final logical step in the lethal dialectic, and no one would have faulted Schmuhl if he had not merely alluded to it but fully included it in his book as part 3, but then the volume would have become too large.)

This is a carefully written study, based on original sources in the Koblenz federal archive and two regional Protestant church archives as well as a host of primary and critical literature. As such, it is a contribution to the ever-growing field of the history of medicine and physicians under National Socialism. As I see it, it is one of the most meaningful contributions made in recent times, because of its integrative value. There have been useful detailed examinations of a variety of subjects on which this author touches. For example, the

process of Nazi "euthanasia" itself was described many years ago by Alice Platen-Hallermund and then again, in more detail and on the basis of fresher evidence, by Ernst Klee. Gisela Bock has done an astute analysis of Nazi sterilization, albeit unnecessarily centered on female victims. Geoffrey Cocks has written about the role of some psychiatrists, as they became involved in or turned their backs on "mercy-killing," and Robert Jay Lifton brilliantly analyzed the murder of the Jews as dialectical "stage three." Renate Rissom and Sheila Weiss, in separate monographs, treated biographically two of the unwitting progenitors of "euthanasia," Rissom Fritz Lenz and Weiss Wilhelm Schallmayer. Benno Müller-Hill has pointed out the scientism of eugenicists beholden to Hitler, while, almost contemporaneously with Schmuhl, Robert Proctor has examined "race hygienics" as pervading much of the medical practice in the Third Reich.

The significance of this book is threefold. First, it provides fresh evidence on Nazi eugenic processes, for example, some highly interesting facts regarding the collusion of Nazis and Protestant church representatives in the matter. Second, it clarifies the origins and development of Social Darwinist thought as it merged with eugenic and then racial notions in the *Sonderweg* context of Germany. Third and most important, Schmuhl is the first to show persuasively that Nazi medicine—as exemplified by sterilization and "euthanasia"—did not simply drop from the sky in January 1933 but was the logical outgrowth of theorems and ideological concepts upheld as early as the Wilhelmine empire. Schmuhl has successfully pulled together the various problem areas treated by the authors mentioned above and has provided a perhaps somewhat deterministic, but nevertheless cogent, chronicle of "ideas into politics," the blueprint for selection and counterselection, the master plan and its realization for "Aryan" survival.

MICHAEL H. KATER
York University

ULFRIED GEUTER. *Die Professionalisierung der deutschen Psychologie im Nationalsozialismus.* (Suhrkamp-Taschenbuch Wissenschaft, number 701.) Frankfurt, F.R.G.: Suhrkamp. 1988. Pp. 592. DM 30.

Soon after World War II, a psychologist at the University of Mainz, Albert Wellek, used his position as president of the German Psychological Society to attack a group of Jungian psychotherapists who had recently won state and municipal funding for an institute in Stuttgart. Wellek charged that Carl Jung (a Swiss) and his German adherents had collaborated with the Nazis to a degree far beyond that of the very few psychologists who compromised themselves and their work under National Socialism. In continuing a longstanding and increasingly acrimonious debate, Wellek was indulging in a self-serving fantasy: that the history of the development of his discipline could be detached

from that of the Third Reich. It is true that psychology, unlike Freudian psychoanalysis, was not officially suppressed by the Nazis and that it also was not simply Nazified in theory or practice. But the reality was more complex and, if anything, more unsettling: the normal process of professionalization of an emerging discipline proved compatible with the aims and needs of the Nazi dictatorship. Once the field had been purged of Jews and political opponents, it was business as usual, only better, for psychologists in the universities and technical institutes, in industry, and especially (until 1942) in the armed forces.

Ulfried Geuter's book on the professionalization of psychology in Germany under National Socialism, originally published in 1984, is a model of scholarship: impressively documented, critically astute, and clearly written. Soon to appear in translation from Cambridge University Press, this monograph is an important example of the best new work on the surprisingly tangled domestic affairs of Adolf Hitler's Germany. Geuter, drawing principally on the work of Mitchell Ash on psychology in Germany before 1933, shows how academic psychologists used demand for their services, in particular from the Wehrmacht, to establish professional status in the face of opposition and competition from university philosophers and psychiatrists. The centerpiece of that achievement was the *Diplom-Prüfungsordnung* of 1941, which gave state recognition to psychology as a separate discipline within the university curriculum and set procedures for the certification of psychologists for service in the many realms of modern German industrial society clamoring for such expertise. Although Geuter details a lack of initiative from industry and shows that, in the upheaval of total war that began in 1942, the army jettisoned its psychological testing apparatus, the institutional demand for specialists in the social channeling of human energy and aptitude persisted into the postwar era.

Geuter thus establishes significant dynamic continuities between the Nazi period and the preceding and succeeding eras in modern German history. Those continuities were institutional as well as intellectual and ideological, broadly Western and industrial as well as peculiarly German (for example, characterology) and preindustrial. Geuter also demonstrates that any notion of scientific progress in the age of "big science" must concede an inherent tendency toward the sacrifice of professional ethics and autonomy in favor of economic advancement and service to reigning political and economic elites, trends that were exaggerated under National Socialism but were not unique to it.

GEOFFREY COCKS
Albion College

HARTMUT SCHUSTEREIT. *Vabanque: Hitlers Angriff auf die Sowjetunion 1941 als Versuch, durch den Sieg im Osten den Westen zu bezwingen*. Herford, F.R.G.: E. S. Mittler und Sohn. 1988. Pp. 184. DM 39.80.

Hartmut Schustereit's primary goal in this volume is to show that Adolf Hitler's military campaigns through 1941 were based not on a step-by-step program but on opportunism. Therefore, in the author's view, the so-called step plan (*Stufenprogramm*) set forth by the prominent West German historian Andreas Hillgruber is incorrect. Although Hillgruber's thesis has been questioned before, Schustereit's refutation is different in that his argument is based on the constant changes in economic priorities that took place in Germany's armaments program between mid-1940 and the end of 1941. According to Schustereit, those changes did not allow the already overburdened German industrial base sufficient time to adjust and prepare the necessary weapons and equipment for the attack against the Soviet Union. By using a wealth of war production statistics, from data on mortars and cannons to information on tanks and aircraft, Schustereit demonstrates convincingly that Hitler lacked not only an overall armaments plan but a comprehensive war plan as well. Although racial and expansionist elements continued to play a part in his thinking, his initial impetus for invading the USSR was his belief that defeating the Soviets would ease the eventual triumph over still-unvanquished Britain. The route to London lay through Moscow.

Although Schustereit's contention is well argued, his work is still subject to some criticisms. For one thing, to make his point, he overstates the invalidity of Hillgruber's thesis. It remains a valuable heuristic device to explain Hitler's essential goals of racial supremacy, additional living space, and world dominion. What is invalid about Hillgruber's heretofore generally accepted notion is his contention that Hitler had a systematic, step-by-step plan for carrying out his goals. Schustereit could also have used the research of other historians to buttress his argument. He emphasizes Bernd Stegemann's criticisms of Hillgruber's *Stufenprogramm*, but Gerhard Weinberg, Milan Hauner, and Martin van Creveld have also implicitly and explicitly pointed out flaws in Hillgruber's idea, though not in the precise economic context used by Schustereit.

Finally, the book's four chapters do not fit well together. In chapter 1, Schustereit analyzes the abrupt changes in Germany's military economic policies during the period 1940–41, the lack of a unified military budget, and the difficulties inside the Reich because of insufficient raw materials and skilled workers. Those themes are linked to Schustereit's detailed criticism of Hillgruber's thesis in chapter 3, but chapter 2, which describes the Wehrmacht's personnel and material losses on the eastern front from June through December 1941, is not integrated adequately with either the changes in economic priority or the author's rebuttal of Hillgruber. And chapter 4, a discussion of the team approach to writing history and how it is used by the historians composing the widely acclaimed *Das Deutsche Reich und der Zweite Weltkrieg* series, though interesting in its own right, would have been better published elsewhere rather than as part of this book. Neverthe-

less, in the continuing debate about the nature of Hitler's war aims, Schustereit has made a significant contribution by stressing the economic dimension of those aims.

ALAN F. WILT
Iowa State University

RONALD W. ZWEIG. *German Reparations and the Jewish World: A History of the Claims Conference*. Boulder, Colo.: Westview. 1987. Pp. xi, 198. \$32.50.

In November 1949, two months after becoming the first chancellor of the Federal Republic of Germany, Konrad Adenauer declared his government's willingness to make restitution to the Jewish victims of Nazism. It took, however, nearly three years of protracted and delicate negotiations before West Germany signed its first international commitment to pay reparations and indemnification to individual Jews, to the state of Israel, and to Jewish communities around the world. On the German side, Adenauer had to contend with the reluctance of the German political elite, even though he early on made clear his refusal to accept an unlimited or open-ended commitment. On the Jewish side, there was more than the moral question of whether to negotiate at all with Germans so soon after the horrors of the Holocaust. No less problematic was the task of forging a body that could represent the claims of all of the survivors, now scattered world-wide. Largely as a result of the initiatives of the new state of Israel and the leadership of Nachum Goldmann, president of the World Jewish Congress, the Conference of Jewish Material Claims against Germany was established. Ronald Zweig's short but lively account is largely drawn from the archives of this claims conference, and Zweig seeks to evaluate the impact of its activities on the reconstruction of the Jewish world and in particular of European Jewish communities.

Apart from the claims of individual survivors and the separate intergovernmental negotiations, the claims conference sought a sum of \$500 million as recompense for plundered Jewish assets for which no heirs could be traced. The Germans offered only DM 450 million, in goods supplied to Israel for the relief of hardship cases. The conference was in fact left with only an annual income of some \$10 million, most of which was preempted for individual relief.

As Zweig admits, the contribution of the claims conference to the rebuilding of the Jewish communities of Europe was at best marginal. All had been tragically reduced in size, but some, like that of Amsterdam, soon recovered. Others in Eastern Europe were prevented by their Communist governments from receiving outside help. The conference showed no interest in reviving any community in Germany. Nevertheless, by favoring established welfare agencies, the conference did its best to allocate its limited resources wisely. Zweig's verdict is positive. As a temporary expedient for transferring West German funds to the Nazis' victims, the

conference operated satisfactorily. But its organizers' hopes of reconstituting the Jewish communities of Europe to their pre-Hitler heights were never realized. Zweig's orderly account of the political and administrative problems faced by the conference, until its dissolution in 1964, is a useful contribution to our knowledge of Jewish survival in the first two postwar decades.

JOHN S. CONWAY
University of British Columbia

MARK CIOC. *PAX ATOMICA: The Nuclear Defense Debate in West Germany during the Adenauer Era*. New York: Columbia University Press. 1988. Pp. xxii, 251. \$27.50.

This is a splendid little book. It illuminates for the first time a side of West German history that was at the very heart of German political-moral life in the era of Konrad Adenauer but that has as yet received little notice in the United States. The stormy debates over rearmament and nuclear weapons reveal a Germany beyond the materialism of the economic miracle and the submissive loyalty of the U.S.'s staunchest cold war ally. They lay bare the central dilemma of West Germans in the 1950s, when they were caught between the trauma of their own past and the very real threat from the Stalinist East. A great majority of West Germans clearly desired a strong military shield. Yet they were equally afraid of a nuclear holocaust on their soil, and many were unsure whether they could trust their own military leaders after the Nazi catastrophe of World War II.

Mark Cioc documents the nuclear debate with great skill, objectivity, and empathy. In three sections reminiscent of a dialectical structure of thesis, antithesis, and synthesis, he first analyzes the Western policy of forward defense as it emerged in the 1950s and the accompanying West German rearmament debate. His objective critical analysis makes both U.S. political decision making and West German fears of becoming American cannon fodder plausible for the reader. Cioc captivates his audience most fully in the longest and best section of the book about the peace debates in the scientific community, churches, and the socialist-liberal Left, where he communicates a genuine empathy for the moral concerns of the main protagonists yet remains critical of the poor arguments and manipulative politicking of the Communist agitators, who ultimately destroyed the antinuclear movement in Berlin in 1958 in full harmony with Nikita Khrushchev's ultimatum. Ultimately, Cioc's sympathy lies with the "synthesis" of a tripartisan defense policy, successfully promoted by the Social Democratic reformers, or "Young Adenauers," as he calls them, who as moral realists accepted Adenauer's concept of a German contribution to a combined flexible nuclear and conventional Western defense in order to ensure a thoroughly democratic German army and genuine efforts toward détente within a framework of an effective Western defensive position. Each step in the grand debate, Cioc points

out, led to an increased number of more destructive nuclear weapons on German soil.

Notwithstanding small irritations—such as an apparent confusion of the effect of Sputnik and intercontinental ballistic missiles (p. 47); the description of Franz-Josef Strauss as a “Prussian” warmonger (p. 118), forced by the *Spiegel* affair to withdraw from federal politics for “nearly a decade” (p. 183, the correct total is three years); and the portrayal of Theodor Heuss as the author of “the West German constitution” (p. 123)—this is an excellent book, which captures the drama and mood of the nuclear defense debate and the German political atmosphere quite as it appeared to the contemporary observer. Skillfully selected and abridged documents interspersed in the text, a fine assortment of characteristic cartoons, and a graceful writing style make this study a pleasure to read and most suitable for use with students.

DIETHELM PROWE
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SANDOR AGÓCS. *The Troubled Origins of the Italian Catholic Labor Movement, 1878–1914*. Detroit: Wayne State University Press. 1988. Pp. 251. \$29.95.

Sandor Agócs deals with an interesting subject and investigates an important topic virtually untouched in English-language works. Researchers who wish to survey the origins of the Italian Catholic labor movement, its intellectual underpinnings, the positions of competing camps, and the predominant role of the popes in labor discussions will find this work useful.

One cannot read Agócs's book, however, without coming away unsatisfied and frustrated. First, this is a book that was born obsolete. Even in his conclusions, the author misses the opportunity to give a broader view of his material. The “mixed” union of employers and employees that was pushed by Catholic conservatives is a major theme of the book, but there is no follow-through—no hint of any possible connection with the fascist corporate state, for example. The same might be said about a number of other issues.

Perhaps a reason for the lack of follow-through is a failure that also damages the body of the work. The age of the secondary works cited by Agócs indicates that he is unfamiliar with the latest research on many of the topics he discusses. The most recent secondary work that I could find in the bibliography dates from 1978. In addition, the bibliography itself is unimpressive. For example, only one of Giovanni Spadolini's numerous works on the Catholics and Liberals of Italy is mentioned. Giorgio Candeloro's book on the Catholics, which might have provided a broader perspective, is not cited.

Clearly, this weakness could not have been corrected by adding a book here and there. Although on the major theme the author's research in primary sources and older secondary works has to some extent saved him, the same cannot be said about the all-important

discussions regarding Catholic relationships with other labor movements and the government.

The author's treatment of the Socialists, the founders of the modern Italian labor movement, is particularly poor. For example, the Socialists' supposed hostility toward peasant smallholders and the thesis that the Socialists did not attempt a linkup between workers and peasants until V. I. Lenin advised them to do so after World War I are nonsense (pp. 157–58). Like the Belgian movement, Italian socialism pioneered in organizing peasants, a fact to which party documents, Camillo Prampolini, and the explosions during 1901–03 in the number of agricultural leagues and of successful strikes on the land all testify. Characteristically, on this point Agócs cites a history of socialism published in 1912. Since, according to the author, the failure of the Socialists to organize smallholding peasants created a niche for the Catholics, something else must account for the growth of Catholic unions. The author's discussion of Gaetano Salvemini's ideas is similarly shallow.

In short, not only did the author fail to consult primary documents on the Italian Socialists, but he ignored significant recent research on them as well. That oversight accounts not only for his mistaken views on the social history of the period but also for his lack of depth in the political history—for example, his failure to emphasize the 1904 general strike as the occasion for a partial lifting of the *non expedit*.

Finally, the same weakness pervades the discussion of Catholic relations with the ruling Liberals and Giovanni Giolitti. Despite the author's apparent belief (p. 117), Giolitti pursued a fairly enlightened policy toward workers. And, despite surface hostility, “underground” cooperation with the Catholics prevailed—in the government's failure to enforce anticlerical legislation, in its favoring of Catholic schools, and its hostility to divorce. Had the implications of those issues been fruitfully discussed, the book would have gained in significance.

SPENCER M. DI SCALA
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Boston

WILLIAM A. RENZI. *In the Shadow of the Sword: Italy's Neutrality and Entrance into the Great War, 1914–1915*. (American University Studies, series 9, number 26.) New York: Peter Lang. 1987. Pp. xvi, 359. \$59.50.

Italian intervention in the Great War has been examined in Italy and abroad since the “radiant days” of May, producing a vast printed literature, part of which has been catalogued in this book's bibliography. After the works of Luigi Albertini, R. J. B. Bosworth, Corrado De Biase, Giorgio Rochat, Mario Toscano, and John Thayer and the articles of William A. Renzi himself, among others, one must question the need for yet another work on the subject. I found no justification in the preface, but a paragraph on the back cover

sought to provide an explanation. This blurb alludes to the use of hitherto unavailable sources, suggesting that the study is unique in detailing the diplomacy that moved Italy from neutrality to intervention, while accounting for the domestic and international factors that determined the course of events.

Undeniably, Renzi has based his conclusions on a broad range of primary source material, including official archives in Austria, France, Germany, the United States, and Italy. He has also plumbed the private correspondence and memoirs of the principal protagonists in the drama: those in the Entente as well as the Triple Alliance. He has also put to good use the published documents and official histories of the major belligerents, including Russia, placing Italian actions within the framework of international developments. Although his findings generally tend to corroborate those of the earlier studies with little revision, his assessment, contrary to the general consensus of contemporaries and subsequent historians, that Italy gained more from its participation in the Triple Alliance than either Austria-Hungary or Germany (p. 5) is unconvincing. Still, his delineation of the events that culminated in Italian intervention is solid diplomatic history.

Like all good diplomatic history, this volume delves into the domestic factors that contributed to international relations. Renzi's foray into the domestic well-spring of Italy's prewar diplomacy is, however, at times weak. Thus, in the first three chapters, which focus on background material, Giovanni Giolitti, the man who dominated the country's political life during the prewar decade, is given short shrift. Indeed, his foreign minister, Antonio Di San Giuliano, is seen to play the key role in the decision to move into Libya (p. 16) as well as in the government's essentially neutral policy. I grant that Giolitti concentrated on domestic concerns, but he appreciated the interrelationship between internal and international relations and required peace to tackle the country's massive social, political, and economic problems.

Renzi's analysis of Italian policy following the Sarajevo crisis is better, and here he makes some contributions. For example, Di San Giuliano's emphasis on Article 7 of the alliance agreement is shown to be motivated as much by the desire to encourage moderation in Vienna as by the desire for territorial aggrandizement (p. 72). Moreover, Renzi illustrates that in the weeks before his demise, Di San Giuliano, who remained in the Salandra government at Giolitti's insistence, had definitely opted for intervention against the Dual Monarchy (p. 100). Little new light, however, is shed on Giolitti's surprising inactivity in the face of the intervention he opposed. Finally, Renzi seems confused regarding the role played by King Victor Emmanuel. At one point he sees the king repudiating the monarchy's constitutional and extraconstitutional powers, describing his desire to reign but not rule (p. 191), but later Renzi reveals that the king was not only

interested in intervention but also helped bring it about (pp. 247–57).

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New York

D. PRODAN. *Răscoala lui Horea* [The Horea Rebellion]. In two volumes. 2d rev. ed. Bucharest: Editura Științifică și Enciclopedică. 1984. Pp. 622; 771.

In the "Age of Ceaușescu," when Romanian historiography links the historic past of the Romanians to the historic present through ideologically motivated revolutions and nationalism, and at a time when *Erdély Története*—the massive history of Transylvania published by the Hungarian academy—rekindles many a controversy, it is refreshing to study the masterful *Răscoala lui Horea*, the chef d'oeuvre of the great Romanian historian D. Prodan. Prodan's exhaustive study of the revolt of 1784 in Transylvania and of its significance in the history of the Romanians has two outstanding merits. First, it is based on all primary sources accessible to an author who spent much of his distinguished career in search of the data; second, it provides a sophisticated, thoroughly objective analysis of the evidence that leads to rational conclusions.

The nearly fourteen hundred pages of the work are devoted to the origins of the revolt, in the context of the history of Transylvania and of feudalism in the age of Joseph II, to the revolt itself in all of its aspects, and to the consequences of the events of 1784 in the broader context of the history of reforms, revolutions, and reactions in late eighteenth-century Europe. Prodan's narrative, in which he often renders the rather difficult prose characteristic of eighteenth-century Romania, is remarkable. He conveys the mentality of the combatants and depicts the *couleur locale*. His attention to relevant detail also makes the work outstanding. In truth, Prodan adheres to the guiding principle of Romanian historians of his generation, that is, *actele vorbest*e (the documents speak for themselves.)

Important as the minute record of events and the interplay of protagonists may be to students of Transylvanian and Romanian history, the greatest value of the study rests in Prodan's restatement of conclusions reached by Hungarian, Romanian, and other historians and politicians for whom the revolt of 1784 has provided inexhaustible ammunition for intemperate controversies and polemics. Prodan explodes myths and misconceptions. First, he demonstrates that Horea's revolt was unequivocally a peasant revolt whose goals were simple and reflective of the aspirations of the masses: the abolition of the aristocracy and of serfdom. He rejects the dubious contentions advanced by less well-informed writers that other groups, such as Freemasons, foreign agents, Romanian nationalist intellectuals, were indispensable for providing an ideological basis for Horea's revolt. He also dismisses as

baseless the often-held view that the revolt was encouraged by Joseph II, who presumably used Horea as an instrument in his struggle with the Magyar aristocracy. The invocation of an "imperial mandate" by Horea is explained by the need to claim legitimacy for the revolt and its leader. Prodan derides the contention that Horea sought to become the *Rex Daciae*, as accused by his enemies, or that he, as "prince" or "emperor" of the rebels, sought to transform the peasant uprising into a national revolution. It is precisely the redefinition of the national character of Horea's revolt that singles out Prodan's study from all others on the subject. Prodan rejects the ever-popular view that the revolt was a national one directed against the Magyar aristocracy or, for that matter, that it was a conscious forerunner of the revolutions of 1848 and, as such, of the common struggle of all Romanians for independence and unification in a greater Romanian state. Instead, he stresses the social revolutionary character of the events of 1784, while recognizing the fact that the revolt was also "national" inasmuch as the vast majority of the rebels were Romanians concerned with the abolition of serfdom and the destruction of a non-Romanian, non-Orthodox aristocracy. Prodan is particularly anxious, however, to place that "peasant nationalism" within the historic experience of the Romanian peasantry rather than that of the Romanian intellectuals of Transylvania or that of the Romanians of Moldavia and Wallachia. The rebels, according to Prodan, ultimately sought liberty and the termination of serfdom, and there can be little doubt that this conclusion is rooted in the extensive evidence and analysis that characterize this magnificent work.

STEPHEN FISCHER-GALATI
University of Colorado

LARRY WOLFF. *The Vatican and Poland in the Age of the Partitions: Diplomatic and Cultural Encounters at the Warsaw Nunciature*. (East European Monographs, number 245.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1988. Pp. x, 282. \$35.00.

If misery loves company then the late eighteenth-century papacy and Poland were made for each other. While the Roman church was being assailed by the philosophes, reforming monarchs, and French revolutionaries, Poland was literally being torn apart by domestic chaos and the foreign partitions of 1772, 1793, and 1795. To Poland's last three papal nuncios—Giuseppe Garampi (1772–76), Giovanni Archetti (1776–84), and Ferdinando Maria Saluzzo (1784–94)—fell the daunting task of confronting both sets of challenges. In their travails the nuncios and their Polish hosts shared much common ground. Just as the Poles mourned each partition as a national tragedy, the church dreaded the transfer of millions of Catholics to the dominion of Orthodox Russia and Protestant Prussia. The suppression of the Jesuits in 1773 was not only

very unpopular inside Poland, it proved especially humiliating for the papacy when both Russia and Prussia refused to enforce the suppression decree in their Polish lands because they did not wish to dismantle the Jesuit school system. If the threat of aggression ultimately compelled the Poles to curtail their precious freedoms and raise an army, it also struck at the church when the Polish diet voted to finance that army by confiscating the wealth of all vacant benefices. And the final Russian invasion that spelled the end for Poland brought with it domestic turmoil that led to the violent death of several Polish churchmen, including the country's archbishop primate. The nuncios in Poland found so much of their work distasteful that they yearned for an early transfer to another assignment. They were overwhelmed by the country's severe climate, cultural backwardness, and incomprehensible language. They distrusted its "apostate" king (Stanislaus Poniatowski), feared the all-powerful Russian ambassador, abhorred the insane bishop of Cracow, and positively dreaded the chaotic Polish diet, where Garampi described his work as "continual martyrdom" (p. 63).

Yet what Larry Wolff finds most fascinating about this story is not the trials and failures of the papal nunciature but the successes that it engineered in the face of difficult circumstances. He finds that the nuncios "were extremely capable, sometimes genuinely brilliant, ecclesiastical diplomats who created an intricate and remarkably effective policy out of the scantest diplomatic means" (p. 249). In recognizing the inevitability of foreign intervention and partition, they were generally successful in salvaging the church's position in the alienated territories and in excluding non-Catholic dissidents from power within rump Poland. Although they failed to counter the spread of revolutionary and nationalistic currents on the eve of the last Russian invasion, Wolff argues that through deft maneuvering they managed to preserve the church's close relationship with the Polish people.

The book's greatest strengths and weaknesses lie in the author's heavy reliance on the nuncios' reports. Although Wolff presents us with new and detailed information about the nuncios' comings and goings and about the ins and outs of their intrigues, he sometimes does so at the expense of fuller background, proper stage setting, and a broader diversification of sources. That flaw is especially evident at the end of his story, when the nuncios' records (and hence his coverage) of the last two partitions are very skimpy. Wolff does provide us with an excellent conclusion in which, among other things, he draws some provocative parallels with today, including the church's need to deal with a Poland dominated by the Soviet Union and the church's sensitivity to the intertwining of Polish piety and nationalism. Indeed, he argues that the nunciature's adept diplomacy during the partition era laid the foundations for "the 'link of love' hailed by Pope John Paul II, the first steps towards a more striking solidarity" (p. 249) between the pope and the Polish people. Such grandiose visions aside, this book should prove

useful for specialists, though probably less valuable for scholars interested in obtaining a broader view of eighteenth-century Polish history.

CHARLES INGRAO
Purdue University

AGNEZA SZABO. *Središnje institucije Hrvatske u Zagrebu 1860–1873* [Central Institutions of Croatia in Zagreb, 1860–1873]. Volume 1. *Društvena struktura nosilaca političkih i privrednih institucija* [Social Structure of the Leaders of Political and Economic Institutions]. (Zavod za hrvatsku povijest, Monografije, number 14.) Zagreb: Zavod za Hrvatsku Povijest. 1987. Pp. 240.

With this volume Agneza Szabo has inaugurated a project that promises to be a major contribution to nineteenth-century Croatian historiography. With great diligence and effort, but apparently without the aid of a computer, she has begun a study of the social structure of that group of individuals involved during the years 1860 to 1873 in what she calls the central institutions of Croatia in Zagreb, including the Sabor, the judiciary, the bureaucracy, the press and journals, economic institutions, cultural organizations, the university, and the academy. She calls her work the first quantitative study in Yugoslav historiography—a claim that, as far as I know, is correct. As such this study is important, but it is significant as well because it helps shift the focus of modern Croatian historiography toward relatively neglected institutional issues.

The organization of this volume is simple. A list of all fifty-six institutions included in the study (only thirty-eight of which Szabo discusses in this volume) precedes her treatment of each of those institutions that she has labeled political or economic. For each she provides background information; the list or lists of people associated with it, with indications of how long and in what role each person served; and a description and analysis of the list. At the end of both the political and the economic sections she provides a brief comparative analysis of all the lists in that section.

Szabo's conclusions are cautious ones. She tries, with mixed results, to assess the extent to which participants in one political or economic institution participated in another institution of the same kind, but she does not, at least in this volume, correlate political participation with economic. She also tries to determine the average number of officials in various government offices, the officials' average tenure, and their movement among offices. Her conclusions here, as she herself admits, are weakened by lack of data for the years 1867 through 1869. She does detect a major increase between 1860 and 1873 in the number of people involved in economic institutions.

If Szabo's conclusions seem meager compared to the effort involved in the task of compiling and analyzing the data, it is because this volume contains only partial results, and those results deal with only the political and economic institutions and their associated press

and journals. The publication of the projected second volume, covering the cultural institutions and the press, a survey of the activity of all of the individuals studied, and a consideration of the "structure of the leading group" of participants, was delayed "because of financial reasons" (p. 13, n. 1).

Without the second volume the results of this study are hard to assess. The potential benefits, however, are great. Szabo has had to assemble a vast amount of data, often concerning people about whom little is known. Included in the data is a master list, keyed to all the shorter lists, of every name she encountered. The master list could and should be used to trace the activities of individuals as well as groups. Let us hope that Szabo's second volume will fulfill its potential. Let us hope, too, that she will continue such research.

JAMES P. KROKAR
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JAROSLAW PELENSKI, EDITOR. *The Political and Social Ideas of Vjacslav Lypyns'kyj*. (Harvard Ukrainian Studies, volume 9, number 3/4.) Cambridge: Ukrainian Research Center, Harvard University. 1985. Pp. viii, 237–508.

Viacheslav Lypyns'kyi (1882–1931) was the outstanding exponent of Ukrainian conservatism. His self-identification as a Ukrainian was purely a matter of personal choice, since he was born an ethnic Pole. Unlike earlier Polish noblemen who made the same choice, such as the historian Volodymyr Antonovych, Lypyns'kyi was not moved to adopt Ukrainian nationality by any particular sympathy with the oppressed Ukrainian peasant class. He justified his passage to Ukrainian nationality by the circumstance that he was born in the Ukraine, in Volhynia; his conservative ideology put forward a conception of nationality that was new for Ukrainians in that it stressed the territorial factor over such traditional ethnic markers as common descent, language, and religion (Lypyns'kyi remained a Roman Catholic all his life).

Lypyns'kyi first came to prominence in Ukrainian society before World War I, not as a political thinker but as a historian. At a time when Ukrainian historiography was dominated by a populist tradition, indeed, when the lion of Ukrainian intellectual life was the consummate populist historian Mykhailo Hrushevs'kyi, Lypyns'kyi radically broke with the prevailing doctrines and began to write about the positive role of the nobility in Ukrainian history, particularly during the great Cossack revolt against Poland of the mid-seventeenth century. During the Ukrainian revolution and civil war of 1917–20, he was one of a very small number of Ukrainian intellectuals to support the government of Hetman Pavlo Skoropads'kyi, whom most Ukrainian activists considered a German puppet and brutal suppressor of the peasantry. In the wake of the revolution, which alienated Lypyns'kyi profoundly, he wrote the fundamental work of Ukrainian conservatism, *Lysty do brativ-khliborobiv* [Letters to Brother Farmers], a treatise

of some five hundred pages. His work had a major influence on the émigré followers of Hetman Skoropads'kyi and on Ukrainian Catholic circles in Eastern Galicia, then under Polish rule. Many of his ideas were borrowed, transformed, and applied to different ends by the ideologue of the emerging Ukrainian radical right, Dmytro Dontsov, for whom Lypyns'kyi had nothing but contempt. Lypyns'kyi died of a chronic lung disease in an Austrian sanatorium just as the radical nationalist Right became hegemonic in Ukrainian Galicia. Disgust with nationalist politics in the 1930s and during World War II led a number of prominent Galician Ukrainian intellectuals to return to Lypyns'kyi's legacy and to seek inspiration in his order-seeking, elitist doctrines. It is essentially those intellectuals who have composed the anthology under review.

The volume is somewhat unusual for its genre. It owes its origin to a conference on Lypyns'kyi held at Harvard University in 1982, but it is far more than a collection of conference proceedings. Instead, creative editorial work has produced a very interesting reader consisting of three parts: scholarly articles, primarily deriving from the Harvard conference and all of high intellectual caliber, investigating aspects of Lypyns'kyi's thought; a series of previously unpublished articles and letters by Lypyns'kyi, each illuminating some facet of his politics from a fresh perspective; and a rich, varied florilegium of older, largely publicist pieces devoted to Lypyns'kyi, republished—with one puzzling exception—in the original Ukrainian and Polish versions. The book opens and closes with very handy reference materials, the editor's introduction to the collection, and a chronology and bibliography at the end. The volume reflects the high editorial standards of *Harvard Ukrainian Studies*, of which it is a special issue.

Although this book is a noteworthy intellectual achievement, it is definitely the work of the faithful; nonbelievers will be sometimes amazed, sometimes amused by what seem to be exaggerated piety and inflated praise. Moreover, shared assumptions and doctrines have evidently prevented the contributors from explaining certain mysteries to the uninitiated. I remain puzzled, for example, how Lypyns'kyi could not only believe but also make a cornerstone of his political interpretation of the Ukrainian revolution the notion that the Ukrainian intelligentsia was responsible for the peasant rebellions that swept across the Ukraine during and after the reign of Hetman Skoropads'kyi. Which Ukrainian intellectual was the evil genius behind Nestor Makhno? Still, this is a fine book, and it makes the best case for Lypyns'kyi that has been made. It is a major contribution to East European intellectual history and should also interest historians of European conservatism in general.

JOHN-PAUL HIMKA
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JERZY TOPOLSKI. *O nowy model historii: Jan Rutkowski (1886–1949)* [On the New Model of History: Jan Rut-

kowski (1886–1949)]. Warsaw: Państwowe Wydawnictwo Naukowe. 1986. Pp. 310. 400 Zł.

Jan Rutkowski was one of the most prominent Polish historians in the first half of the twentieth century. Influenced to some extent by Marc Bloch and the French Annales school of economic and social history, he established a reputation in his homeland and abroad long before the introduction of Marxist historiography at the Seventh Congress of Polish Historians held in Wrocław in September 1948.

Rutkowski is the primary focus of Jerzy Topolski's book. It consists of two major parts almost equally divided (five chapters each). Part 1 is a personal and scholarly biography of an unassuming intellectual with high moral principles and an innovative approach to the study of history. Informative and succinct, this section is based on a variety of sources. These include French and Polish archival materials, family papers and interviews, and printed materials.

The author introduces Rutkowski's "bourgeois-szlachta" family background. His father was a pioneer in Poland's pharmaceutical industry. Topolski discusses at length Rutkowski's studies in Poland and abroad and the formative influences of Ludwik Finkel, Szymon Askenazy, Henri Berr, Henri Sée, and Bloch. He describes the establishment of Rutkowski's international reputation as an economic historian in the period between the two world wars. Rutkowski emerges as a resilient man with a remarkable ability to avoid despair and to function as a productive scholar in spite of the adversities of war.

Especially interesting is the period 1945–49, when Rutkowski returned to the university in Poznań. Assigned to conduct a seminar and faced with a serious lack of documentary sources, he resourcefully gave his students the task of interviewing survivors of the war and Nazi occupation. Using memory as a primary historical source, the students compiled new and valuable sources for a recognizably critical period in history (p. 157).

In part 2, Rutkowski's theories and conceptions of history provide the focal point for a competent, sophisticated analysis by Topolski. In chapter 6, he introduces Rutkowski's non-Marxist "theoretical-explanatory" model of history and Rutkowski's ideas concerning the training of historians. The three chapters that follow focus on Rutkowski's theory of historical synthesis, on his interpretation of the theory and method of historical materialism, and on an assessment of Rutkowski's seminal socioeconomic studies of rural, prepartition Poland.

In developing his analysis, Topolski consistently asserts the non-Marxist nature of Rutkowski's work. He devotes much space in part 2 to Rutkowski's Marxist critics—historians who emerged under the aegis of the government at the Wrocław congress in 1948. N. Gasiorowska, W. Kula, and S. Śreniowski have all found fault with Rutkowski's theory of materialism and with his economic history. In each case, Topolski

effectively counters their arguments and reasserts the validity and originality of Rutkowski's non-Marxist interpretation of economic and social history. It is disappointing that non-Marxist contemporaries of Rutkowski's critics are not afforded equal attention.

In a speculative final chapter, Topolski interprets a fragmentary, unpublished manuscript by Rutkowski on evolution, revolution, and socialism. He concludes that the historically based ethical socialism expressed in the manuscript represents a major contribution by Rutkowski to contemporary political thought. Topolski aroused my curiosity. Had Rutkowski lived, would he have retained his integrity and independence from government control (which he exhibited at the Wroclaw Congress), or would he have joined the Marxist historians charged with introducing new standards and approaches to the study of history in close imitation of Soviet models?

Minor weaknesses inhere in this work. Although it includes an index of names, a table of contents, and a photographic essay, it lacks a bibliography. A comprehensive bibliography of Rutkowski's publications would also prove useful. Overall, Rutkowski's life is portrayed with great affection, and his work is astutely analyzed. His contributions to economic and social history remain relevant. They represent an older but still viable alternative to the post-World War II Marxist interpretations of Polish history.

JOAN S. SKURNOWICZ
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JAN T. GROSS. *Revolution from Abroad: The Soviet Conquest of Poland's Western Ukraine and Western Belorussia*. Princeton: Princeton University Press. 1988. Pp. xxii, 334. \$25.00.

Jan T. Gross is the author of two other books dealing with the tragic Polish experience in World War II: *Polish Society under German Occupation: The Generalgouvernement, 1939-1944* (1979) and, with Janina Grudzińska-Gross, *War through Children's Eyes* (1981). He also edited Jadwiga Staniszkis's study, *Poland's Self-Limiting Revolution* (1984).

The present work is most welcome. Although much of the information is familiar to older Poles and to specialists in twentieth-century Polish history, this book contains the first scholarly account and analysis, in English, of the "Communist revolution" in Soviet-occupied eastern Poland between September 17, 1939, and June 22, 1941. The population of this region is generally estimated to have been 13 million in 1939. Of this number about one-third were Poles, one-third were Ukrainians, and the rest were about evenly divided between Jews and Belorussians. It was this mixed population that experienced the first implementation of the Communist revolution, that is, the sovietization of society, outside the borders of the USSR.

Gross divides his book into six studies: "The Soviet Conquest," "Elections," "The Paradigm of Social Con-

trol," "Socialization," "Prisons," and "Deportations." He concludes with a discussion of the "Spoiler State." The documentary base consists of twenty thousand accounts or protocols, written by the survivors of Soviet mass deportations from eastern Poland in 1940-41. (Out of an estimated 1,200,000 deportees, 52 percent were ethnic Poles; of these, about 120,000 managed to leave the USSR for Iran in 1942, with the Polish army led by General Władysław Anders.) These documents, written in response to questionnaires, were later deposited in the archives of the Hoover Institute (Stanford, California) and are now in the collections of the Polish government, the Anders Collection, and the Polish embassy, USSR. The author has supplemented those documents with some accounts by non-Poles as well as with quotations from the available local Polish Communist press of the period and, where possible, from Soviet sources.

The most interesting chapter concerns the Soviet organization of "elections" to the national assemblies of the western Ukraine and western Belorussia. (It was published separately in abbreviated form in *Eastern European Politics and Society*, 1 [1987]). The protocols document how the Soviet authorities registered, propagandized, organized, and coerced the whole adult population to participate in an "electoral campaign" lasting from October 4 to October 22, 1939, when the elections took place. Ironically, the protocols indicate that most ordinary people failed to realize that the task of the national assemblies was to vote for incorporating the former Polish territories into the USSR. As the numerous accounts cited amply demonstrate, the election results were rigged to provide a thumping "victory" for the Soviet-appointed candidates. The candidates duly met in the assemblies and "requested" incorporation in the Soviet Ukrainian and Belorussian republics, a request that was graciously granted. The Soviet government has claimed ever since that the population of those territories voted freely to become part of the Soviet Union.

The chapters dealing with conquest, prisons, and deportations contain information just as horrifying as anything we know about the Nazi occupation of western and central Poland—and in some cases worse. When the Red Army came in, the Soviet authorities set out to undermine and break down existing community relations and ties. But, in eastern Poland, "class war" was combined with a "national war," that is, Soviet NKVD officers incited the Ukrainians and Belorussians to "settle accounts" with their Polish masters, thus sanctioning a brief period of controlled terror that led to thousands of deaths. Also, although the Soviets sometimes used the existing local Polish administration and militias for a time, they simultaneously set up new local authorities of their own choice, often made up of known petty criminals. At the same time, they actively encouraged people to denounce each other. In general, there was constant terror, and mass arrests were the order of the day. Gross estimates that by June 1941, one hundred fifty thousand people were in prison;

these were mostly men, so that about 10 percent of the adult male population was behind bars.

Those who survived the dreadful prison conditions and the brutal interrogations were deported with other groups of the population to Siberia in three great waves between February 1940 and June 1941. The author points out that the deportations were not only a Polish tragedy; 48 percent of the deportees were Ukrainians, Jews, and Belorussians. The protocols describe the routine of roundups in the dead of night as well as the overcrowding and lack of food, water, and sanitary equipment in the cattle cars, all of which led to many deaths en route, particularly among the old, the sick, women, and children. Finally, the Soviets shot an estimated one hundred thousand people in "evacuating" their prisons ahead of the advancing Germans. In sum, Gross estimates that the Soviets killed at least four hundred fifteen thousand Poles between 1939 and 1941 (a conservative figure), which is, as he notes, more than three times the one hundred twenty thousand victims (including the Jews) attributed at this time to the Germans in their part of Poland. (The number of victims there was probably somewhat higher than one hundred twenty thousand [see pp. 228–29]).

The author tells us that he tailored his book to the documentary base and focused his attention on "ordinary people." Therefore, he admits to the marginal treatment or omission of many other facets of the Soviet occupation, such as economic measures, policies toward the nationalities, organized resistance, and the role of native Communists and of the creative intelligentsia.

It is not clear, however, whether it was Gross's criteria or his documentary base that mandated the omission of the ruthless Soviet treatment of the Polish intelligentsia, which as he notes, was targeted for destruction—just as it was in German-occupied Poland. It should be noted that the Soviets had exterminated much of the prerevolutionary Russian and non-Russian intelligentsia in the effort to destroy the Ukrainian, Belorussian, and Baltic intelligentsia. Thus, the destruction of this social stratum was part of the Communist revolution and should have had its own chapter in Gross's study.

Furthermore, although the author points out, in the chapter titled "The Paradigm of Social Control," that the Communist revolution in eastern Poland in many ways followed patterns already established during the Soviet collectivization drive, (that is, denunciations, terror, arbitrary exile quotas), it should be noted that denunciations and terror had already been used during the Russian civil war, while exile quotas in eastern Poland generally targeted "hostile elements," such as the intelligentsia, administrative cadres, Polish settlers, prosperous farmers, and shopkeepers (mostly Jews). Also the political-administrative methods and procedures—the setting up and composition of "local government" bodies, school "reform," the organization and manipulation of "elections," and so forth—were a

repetition or adaptation of practices already established in the USSR before collectivization.

The author might also have pointed out that the treatment of "class enemies" in eastern Poland—sudden arrest, brutal interrogations aimed at extracting "confessions," massacres, and the inhuman methods used in deportation—were modeled on the Soviet treatment not only of "kulaks" but also of political prisoners and other "enemies of the people," such as the alleged "Trotskyites" and their putative supporters in the purges of the 1930s. (These methods were also applied to the Balts and Bessarabians, as well as to the nationalities that Stalin deported in 1941–43, the Volga Germans, Tatars, and so forth.)

These comments, however, are not meant to detract from the overall value of Gross's detailed and fascinating case study of the implementation of Communist revolution in the former eastern territories of Poland in 1939–41. His book should be read by all students of Soviet history, sociology, and government.

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MICHAEL J. ZEPS. *Education and the Crisis of the First Republic*. Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1987. Pp. 241. \$25.00.

Historians still have much to learn about the conflicts that divided Austrians so bitterly between the two world wars. In a labored effort, Michael J. Zeps grapples with one of the most divisive issues of the time: the nature and control of the country's educational system. In the main, Zeps succeeds in highlighting the ideological arguments and the political clashes among contending factions. Unfortunately, his organization and writing style are likely to confuse and irritate even a sympathetic and tenacious reader. Attentive editorial assistance and supervision would surely have made the book more effective.

As it is, the reader might be well advised to prepare for an encounter with this monograph by reading the chapter entitled "Education for Democracy" in the first volume of Charles A. Gulick's *Austria from Habsburg to Hitler* (1948). Gulick provides a much clearer description of Austria's school system and the controversies surrounding it than does Zeps. What Zeps offers is a more detailed, generally well informed analysis from another viewpoint. Where Gulick unabashedly sides with the Social Democrats and their chief spokesman on education, Otto Glöckel, Zeps's sympathies clearly lie with the Catholic cause, or rather with those whom he considers its more "moderate" champions, men such as Ignaz Seipel and Richard Schmitz. Zeps's major archival sources either reflect his orientation or shape his argument—perhaps both. He draws primarily on the papers of Friedrich Funder, Cardinal Friedrich Piffl, and Richard Schmitz and on the records of the

Austrian Bishops' Conference. Not surprisingly, Gulick's champion—Glöckel—becomes Zeps's *bête noire*.

The author builds his study around the generations-old controversy, which he summarizes at the outset, over whether Austria's schools should be a "politicum" or an "ecclesiasticum." After World War I, the issue became a volatile one. Even when Socialists and Catholics could reach compromises on other issues, they found it almost impossible to do so with regard to this aspect of cultural life. Moreover, differing attitudes and priorities among Catholics, not all of whom shared clerical aims for education, complicated the issue, as did the secular but anti-clerical policies of the Pan-Germans. Although Zeps emphasizes the Catholic-Socialist struggle, he also traces, in a subsidiary theme that warrants a subtitle, the intra-Catholic squabbles among bishops, party, and lay associations that significantly affected the larger conflict. Understandably, Zeps laments the failure of the writers of the constitution of 1920 to reach a consensus on church-state relations, which resulted in the decision to leave "all the old [school] laws . . . in force along with their unresolved conflicts" (p. 42). In Zeps's view, it was this "gap" in the constitution that gave subsequent debates on educational matters their bitter and uncompromising edge.

There is no doubt much truth to this contention. But Zeps isolates and overemphasizes the controversy over education. As informative as his work is, it does not entirely convince the reader that the school issue—ultimately, the issue of church-state relations—played any more weighty a role in the destruction of Austrian democracy than did other areas of disagreement and competition. Neither does it prove that Socialists were any more culpable in the failure to find lasting compromise over the educational system—much less in the failure to save the republic—than were Catholics. But such assertions as these are rightly the stuff of continuing historiographical debate. What Zeps does demonstrate is that historical "truth" is multifaceted. Anyone wanting to understand the quarrel among Austrians over education must now examine Zeps's book as well as Gulick's powerful study.

Whatever his partisan proclivities, Zeps concludes his book with a brief, positive epilogue on the *modus vivendi* that Austrians hammered out after the Nazi nightmare. The final sentence illustrates Zeps's basically pluralistic values: "A mutual understanding of interests, though conflicting, goes a long way toward realizing the democratic ideal of unity in diversity for which modern Austria strives" (p. 191).

C. EARL EDMONDSON
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DAVID RANSEL. *Mothers of Misery: Child Abandonment in Russia*. Princeton: Princeton University Press. 1988. Pp. xiv, 330. \$34.95.

There is a freshness to David Ransel's book that makes it a pleasure to read. The subject matter is different from the standard fare, the materials used have not yet been chewed over, and the angles the author opens up on Russian society are unusual ones. Although the subject may appear to be somewhat narrow, the reader of *Mothers of Misery* should finish the book with a feeling of having gained new insights into an interesting mix of historical issues, mainly relating to what Ransel aptly calls an "unexplored point of contact between educated society and the village" (p. 3).

From an institutional perspective, Ransel's subject may be defined as the history of a relatively autonomous government-sponsored charitable organization, the Imperial Foundling Home (*Imperatorskii Vospitatel'nyi Dom*), and its attempts to deal with such problems as infanticide and child abandonment. There were two central foundling homes in Russia, one in Moscow, one in St. Petersburg. Both were founded during the reign of Catherine the Great (in 1764 and 1771, respectively), and both remained open until the first months of the Bolshevik regime. Ransel tells their story in clear and compelling prose, from their founding until the First World War. (The war years are neglected.) Although I would have liked to learn a bit more about life within the walls of the homes, no significant aspect of their history is left untouched, and many issues are described and analyzed in rich detail. Much attention is devoted to the surrounding countryside, that is, to the villages and especially the villagers whose fates were affected by the foundling homes and whose conduct, in turn, affected the policies of the homes. Thus, the book becomes, in part, a history of rural Russia over a one-hundred-fifty-year period, or, more accurately, of the provincial hinterlands of the two capitals, including parts of southern Finland.

The interaction between city and countryside turns out to be much more than a cliché in this fascinating study. It is presented very concretely as a kind of small-scale socioeconomic system ("network of exchange"), wherein needy villagers, especially women, learned to benefit materially from the traffic in abandoned children unwittingly encouraged by the urban homes, while the homes themselves became so enmeshed in that commerce that they lost their original, lofty *raison d'être*, the enhancement of the imperial family's reputation as an enlightened promoter of healthy, productive, morally sound artisan-citizens. Worse still, by the end of the nineteenth century, a combination of commercial considerations, misunderstanding, and peasant ignorance (though generally attributing the unenlightened practices of their "child-care culture" to systemic factors, Ransel is rather rough on the Russian peasant) prevented the foundling system from accomplishing even the modest goals of reducing infant mortality and the illegitimacy rate, both of which remained extremely high. Major reforms (for example, the abolition of open, undocumented admissions) were introduced in the 1890s but too late to justify the existence of an outmoded system, which

contained some of the same kinds of internal contradictions as the regime that spawned it. Although Ransel gives the founding homes due credit for certain accomplishments (pp. 290–93), one closes the book neither surprised nor upset that they have gone under but greatly enlightened about a range of issues, including peasant mentalities, the thinking about women, children, and maternity at various levels of Russian society, public health policies, and the limits of institutional autonomy in the twilight years of the old regime.

The book is based on careful research in rarely used published sources and unpublished archival material, including the papers of the founding homes themselves. Ransel's extensive European comparisons, in contrast to the token comparisons one finds in many works in the Russian field, are carefully designed, with very specific attention to distinctions of place, period, and religion rather than vague allusion to an undifferentiated "West." Ransel has clearly mastered the relevant secondary literature on France, Italy, and several other countries and puts it to very good use. There are also valuable comparisons between Moscow and St. Petersburg. Charts, maps, and tables are well appointed and easy to interpret. Historians of imperial Russian society should not neglect this readable, innovative, and handsomely produced book on the mistaken grounds that the subject falls too far afield of their more specialized interests.

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The reviewer of David Ransel's book was chosen by a member of the Board of Editors, not by a member of the AHR staff.

E. M. KOSACHEVSKAIA. *N. A. Markevich 1804–1860*. Leningrad: Leningradskii universitet. 1987. Pp. 284. 3 r. 10 k.

In the *Soviet Historical Encyclopedia* (1968, vol. 9, p. 89), there is a brief entry for N. A. Markevich in which his five-volume *History of Little Russia* is dismissed as of little scholarly interest except for the documentary materials collected in volumes 4 and 5. A similarly brief and dismissive entry in the *Ukrainian Encyclopedia* (1962, vol. 8, p. 485) sums up his significance as that of a "compiler" of historical and ethnographic materials. It is the purpose of the present volume to counter this estimate, to indicate something of Markevich's many-sidedness—his activities as a composer, musicologist, poet, critic, cultural liaison between Russians and Ukrainians, statistician, as well as historian and ethnographer—and, above all, to show his links with the radical Russian and Ukrainian thought of his time, that is, to rehabilitate him ideologically.

For this purpose, E. M. Kosachevskaia has examined the Markevich family archives in the Institute of Russian Literature and History (IRLI), the commentaries and estimates of Markevich's contemporaries and near contemporaries, both Russian and Ukrainian, and,

most interestingly and rather unusually for a Soviet scholar, the memoirs of Markevich's talented great-grandson, the French and Italian conductor and composer, Igor Markevich (*Être et avoir été* [1980]). Kosachevskaia has, of course, gone back to N. A. Markevich's own *oeuvre* as well, his published work, his archive, his letters to friends. Although her writing and mode of organization are stereotyped and it would be too much to claim that an exciting figure emerges from these pages, Kosachevskaia is a serious scholar, and her book is not without interest. She rightly emphasizes the importance of Markevich's work as a statistician in charting the demography of the Poltava and Chernigov gubernii. Perhaps she attaches undue importance to Alexander Herzen's use of Markevich's materials in his émigré press, but the fact is interesting in itself. In addition to Markevich's generally recognized importance as a collector of primary historical, ethnographic, and folkloric sources, she points out his musicological importance in collecting Ukrainian folksongs and in composing music for many of the poems of Taras Shevchenko, to whose reliance on and education from Markevich's histories she calls attention.

Part of the point of the ideological rehabilitation, however, is once more to indicate the "appropriate" boundaries of Ukrainian nationalism. Markevich was a "proper" nationalist because of his passionate identification with the ethnic traditions of the Ukraine, with its language and history, its music and folklore, and, above all, with what are viewed as its "popular" causes—rebellion, overt and covert, against the Poles and against oppression by landowners, whether Polish or Russian or the Cossack *starshina*. Kosachevskaia does not try to make a proto-Marxist of Markevich and does admit that his world view had certain class-bound limitations, but at the same time she emphasizes his opposition to serfdom, his work in connection with the preparation for emancipation (which took place only after his death), and his rather tenuous connections with the so-called revolutionary democrats, Nikolai Chernyshevsky and Nikolai Dobroliubov. The main point at issue, however, is his fraternal attitude to Russia. He was a "good" Ukrainian nationalist in that, while he advocated ethnic autonomy, cultural independence, and independence from the economic yoke, he opposed sovereignty and severance from Russia. Bohdan Hmel'nitsky is his hero, I. S. Mazepa his villain. As a historian he was an ardent admirer of Peter the Great in spite of that emperor's abolition of the last vestiges of Ukrainian independence. Kosachevskaia points out that "bourgeois," that is, Ukrainian-nationalist, historians have been hard on Markevich, while populists have praised him. That Markevich, in his early years, was close to Alexander Pushkin's younger brother Lev and possibly had some connection with Pushkin himself is assuredly of some interest. More interesting, however, in my opinion, is the outline this low-keyed book provides for the permissible and the impermissible in

the realm of ethnic nationalism since the advent of *glasnost*.

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S. N. KANEV. *Revoliutsiia i anarkhizm: Iz istorii bor'by revoliutsionnykh demokratov i bol'shevikov protiv anarkhizma (1840–1917 gg.)* [Revolution and Anarchism: From the History of the Struggle of the Revolutionary Democrats and the Bolsheviks against Anarchism (1840–1917)]. Moscow: Mysl'. 1987. Pp. 327. 2 r. 10 k.

S. N. Kanev proposes to demonstrate the "bankruptcy" (p. 252) of anarchism as a philosophy and a political movement and to show that it has little in common with Marxism-Leninism. He assumes this task because of what he assesses as a revival of anarchism and because certain observers have attempted to associate contemporary anarchist-terrorists with the Soviet Union. Kanev believes anarchism to be an enemy of revolution (p. 5). Anarchism, he writes, is the child of bourgeois society, having emerged from utopian socialism.

Kanev supports his thesis in several ways. First, he discusses the theoretical errors of some leading anarchists (Pierre-Joseph Proudhon, Max Stirner, Mikhail Bakunin, Lev Tolstoy, and Petr Kropotkin). Second, he finds evidence that not only the communists but also those Russian writers (Aleksandr Herzen, Nikolai Chernyshevskii, Vissarion Belinskii) deemed their remote ancestors either attacked or rejected anarchist philosophies. Third, he is able to show that nineteenth-century anarchists failed in revolutionary attempts and that many of their Russian followers eventually abandoned the cause. Finally, he outlines the "erroneous" and "harmful" activities of twentieth-century Russian anarchists who failed to understand that democratic revolution must precede socialist revolution, that there must be a period of proletarian dictatorship, and that history declared the time for revolution ripe only in October 1917. The anarchists led efforts to effect rebellion prematurely.

This book is one-sided and tendentious. Kanev picks and chooses facts to protect his theme. It is easy to poke holes in Bakunin's theories and to show that revolution failed at Lyons. It is less honest to insinuate that Bakunin's influence was limited to the deceitful and the disloyal or that Tolstoy's followers abandoned his ideas. Twentieth-century anarchists are evaluated as very weak or inordinately influential (during the July Days, for example) in dizzying rotation. There is little new research here. The book has no index. It seems more suited for required freshman courses at Moscow University than for scholarly use.

There are a few interesting moments. The author provides data about anarchist publications in Russia, 1904–17, that may be useful. There is a charming quotation from Marx on Bakunin, whom Marx really liked in 1864, but then Bakunin is said not to have

become a real anarchist until two years later. The radical writers of the 1860s are portrayed as influenced only by Russian, not Western, thinkers. Why Kropotkin is treated so warmly in one chapter when he becomes an arch-renegade in the next remains a special mystery.

This work abounds in jargon and simplistic categorizations. Everyone has his epithet: "bourgeois democrat," "petit-bourgeois anarchist," "revolutionary democrat," "primitive socialist." Abraham Lincoln, who represented "the most revolutionary part of the American bourgeoisie," is said to have been killed by "a terrorist, an agent of the slaveowners and their cohorts" (p. 141). Bakunin's dialectic was "idealistic and above all recalls the sophistic. His materialism was contradictory and approached . . . vulgar materialism." He "expressed himself as a partisan of historical materialism, but he understood it on the level of economic materialism—a version of vulgar materialism" (p. 119). This book may satisfy readers who enjoy such language and categorization, but it can hardly be recommended to anyone else.

DEBORAH HARDY
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CHRISTOPHER RICE. *Russian Workers and the Socialist-Revolutionary Party through the Revolution of 1905–1907*. New York: St. Martin's. 1988. Pp. xiv, 272. \$39.95.

It has long been assumed that the Russian Socialist Revolutionary party (SR), the party of Victor Chernov and Alexander Kerensky, espoused a form of agrarian socialism and that it was oriented toward the peasantry. Indeed, the revival of interest in this party largely reflects renewed concern with peasants. Yet, despite the party's "peasant" reputation, the SRs enjoyed considerable influence among urban workers, especially during the first Russian revolution of 1905.

In this informative, if not always imaginative, book, Christopher Rice investigates SR activity among workers during this period. Before 1905 the SRs approached workers in a manner reminiscent of their Social Democratic rivals, though with limited success. Only after promulgation of the October Manifesto did the party really involve "the great working masses" (p. 63). St. Petersburg provides an illuminating case study. Here, Rice contends, in late 1905 and 1906, the SRs "were most closely associated with large factories" (p. 109), proving "attractive to workers in the metal industry" (p. 114). But the pattern of SR support could vary, as less thorough and somewhat uneven profiles of SR trade-union involvement and of SR organizations in Baku, the Jewish Pale, Bryansk, Sevastopol, and the Urals suggest. Unfortunately, Saratov, which stood at the center of agrarian unrest led by the SRs but was also a focus of labor organizing, is not examined.

Rice must be commended for making available new and intriguing information about SR activity in the urban grass-roots labor movement. Conceptually, however, his study does not qualitatively improve on,

although it sometimes differs with, Michael Melancon's work on the topic. One problem is the book's relatively narrow base of sources. Although use is made of the SR archive in Amsterdam, the material culled there is least relevant to the author's most arguable conclusions. Those are based, instead, almost exclusively on the SR press and on various secondary materials.

Rice tends to accept too uncritically SR claims to leadership and to exaggerate the structural integrity and political unity of the party, almost in the way Soviet historians have done with Bolshevism. For instance, the All-Russian Railroad Union was not in 1905 dominated by the SRs or any other party, although SRs were active in its leadership and among its members. Working-class SRs and Bolsheviks alike often worked in this union and elsewhere more as individuals than as party agents. It is hardly clear that workers who called themselves SRs espoused a party line or interpreted party affiliation in the partisan spirit of some leaders. The SRs were, by design, more amorphous than their Social Democratic rivals. Yet, even among the Leninists, differences between leaders and the rank and file and within the lower ranks could be dramatic. To be sure, Rice emphasizes the "maximalist" split, but that seems to have been of limited importance among workers.

The book's interpretive arguments, concentrated in a stimulating and thoughtful conclusion, raise intriguing hypotheses that are only partially confirmed by the assembled evidence. Comparing SRs to Social Democrats, Rice concludes that the SRs were more biased toward political activity, less "economistic," than the Social Democrats. But he disagrees with Manfred Hildermeier's contention that failure to overtake the Marxists resulted from an "inherent inability to convert mobilized into organized support." Instead, in an unconvincing and somewhat circular argument, Rice blames a "dearth of activists" (p. 195). According to Rice, labor support for the SRs revealed the political importance of the peasant-worker as opposed to the cadre proletariat. This theory is attractive, but supporting evidence is still limited. Melancon's suggestion that the SRs, like the Mensheviks, appealed to a "labor aristocracy" also seems plausible.

As one of few English-language studies of this remarkable party, this book is certainly welcome. But opportunities for further work remain.

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TERENCE EMMONS, EDITOR. *Time of Troubles: The Diary of Iurii Vladimirovich Got'e; Moscow, July 8, 1917 to July 23, 1922*. Translated by TERENCE EMMONS. Princeton: Princeton University Press. 1988. Pp. xix, 513. \$39.50.

This translation of the diary of Iurii Vladimirovich Got'e provides a fascinating and illuminating account of life in Moscow as a civilized, passionately patriotic

professor described it on an almost daily basis, from July 8, 1917, the day on which Aleksandr Kerenskii replaced Prince Georgii L'vov as head of the Russian Provisional Government, until July 22, 1922. Got'e gave the manuscript to Frank Golder, in Russia working with the American Relief Administration and collecting materials about the revolution for the new Hoover Institution Library, and Golder deposited it with his papers in the library, where Edward Kasinec, now of the New York Public Library, found it fifty years later. With splendid assistance from the Hoover Institution staff, Terence Emmons translated it, prepared immensely useful annotations, selected excellent maps and photographs, and completed a knowledgeable essay on Got'e and his career. This model effort is a great service to scholarship. It is also an act of gratitude and respect: Emmons is one of hundreds of scholars who have benefited from Got'e's research and who have used the manuscript collection of the now Lenin Library that he protected and enlarged. In addition, one of Got'e's students was Petr A. Zaionchkovskii of Moscow State University, an adviser and friend of Emmons and other American specialists in nineteenth-century Russian history and an honorary member of the American Historical Association. The Princeton University Press has honored Got'e and Emmons by producing a very handsome volume.

Got'e was an outstanding member of the Moscow University history faculty and associate director of the Rumiantsev Library and Museum, which became the All-Russian Public Library in 1921 and the Lenin Library in 1924. The collapse of the Russian army and political system stunned and soured him, so the diary is that of an embittered man savagely critical of the Old Regime, the Provisional Government, and Russia's allies. He despised the Bolsheviks, "dog deputies" and "gorillas," and their "Sovdepiia." He assigned Russian national character responsibility for the spiritual and moral crisis that caused the military losses but also blamed the follies of the monarchy and the fanaticism of many intellectuals, determined to use all means to destroy the system. He was critical of Jews, prominent in the two Soviet institutions with which he dealt, the Commissariat of Enlightenment and the Cheka, but Russians, Germans, and Ukrainians also attracted his scorn.

Curiously, Got'e was politically passive, just an observer, but he was resilient and determined to survive and to raise his young son. In the confusion, he concentrated on family and friends, the Rumiantsev Library and Museum, and the university. His labors for the library, especially his work in committees of the Commissariat of Enlightenment and his daily efforts to overcome staff problems and fuel shortages, were fundamentally successful, but he and his colleagues were helpless at the university.

The diary is especially valuable for its description of life as the academic middle class endured it: housing, food and fuel shortages, inflation, the breakdown of transportation, the absence of reliable news (especially

important to Got'e), and the conflicts between administrations and staffs as the established managerial systems and loyalties collapsed. The diary also contains important material concerning the way the Bolsheviks stumbled and fumbled, grappling with the problems power brought them. Got'e provides glimpses into Kremlin political life because of his participation in committees of the Commissariat of Enlightenment, in which an old acquaintance and fellow historian, Mikhail Pokrovskii, played a prominent role. The picture encompasses much of Moscow and provides some insight into other areas, as Got'e carries firewood and food, moves from his cold quarters into the museum, and trudges throughout the city to visit friends and relations, attend funerals, and take crowded trains to Petrograd, Novgorod, Sergiev Posad, and Ivanovo-Voznesensk. Got'e managed to spend long periods of time in the countryside, so the "gentry nest" near the Kremlin is only the center of his account. Above all, the diary shows how family and friendships remained strong throughout the turmoil, providing the base from which Got'e, his colleagues, and most citizens were to move ahead in the 1920s and 1930s. Indeed, the emphasis on family and friends during another tragedy Moscow and Russia endured reminds one of *War and Peace*.

Got'e chose to remain in the Soviet Union, although some of his friends left about the same time Golder carried his diary to Stanford. He was arrested in 1930 and spent four years in a labor camp. He then returned to academic life, published and taught, was elected a member of the Academy of Sciences in 1939, and died in 1943. He clearly was a good person, a splendid representative of old Russia, one of the bridges between the old and the new. Those who read his painful, honest record will achieve sharp insight into the high personal and professional qualities of this sterling scholar and into this Time of Troubles as it afflicted millions of citizens.

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DOUGLAS R. WEINER. *Models of Nature: Ecology, Conservation, and Cultural Revolution in Soviet Russia*. (Indiana-Michigan Series in Russian and East European Studies.) Bloomington: Indiana University Press. 1988. Pp. xiv, 312. \$35.00.

Douglas R. Weiner treats conservation—the main topic of this book—as a complex phenomenon molded by government policies, scientific ideas, cultural values, and ideological tenets. He concentrates on the Soviet period from the October Revolution to the consolidation of Stalinist rule in the mid-1930s. The introductory chapter offers a brief survey of conservation ideas and policies from the age of Peter I to the fall of the tsarist regime. It features the activities of three members of the scientific community: I. P. Borodin, V. G.

Kozhevnikov, and V. I. Taliev. The author is interested particularly in the proliferation of political and scientific designs during the early Soviet period to make conservation an integral part of economic development.

During the 1920s, the conservation movement, backed by eminently successful efforts to transform ecology from a patchwork of descriptive disciplines into a theoretically integrated science, received strong support from both the government and the community of scholars. The Soviet Union was receptive to Western innovations and contributed to the world pool of ecological knowledge. Weiner offers rich information on conservation as a vital tool of government policies, as a critical topic of interdisciplinary research, and as the primary goal of a strong popular movement. In the 1920s, conservation was much more than a utilitarian concern: it was also a topic of philosophical, ethical, and aesthetic consideration.

Ecologists, the successors of a strong national tradition in phytosociology and in the study of biocenosis and biogeocenosis, came from different research backgrounds, but most were attracted to "plant communities" as distinct biotic entities. They provided powerful arguments in favor of national, regional, and local parks as centers of scientific approaches to the riddles of conservation. *Zapovedniki*, the protected territories "maintained in a state of inviolability and devoted to scientific research" (p. xi), are the primary objects of Weiner's fascinating and carefully documented study. Ecologists looked at them as "models of nature" unperturbed by human interference.

Under Stalin, the conservation effort caused cleavages and stark uncertainties. The "bourgeois professors" and various other groups of "wreckers" involved in the conservation movement were exposed to daily attacks by leaders of the proliferating Marxist organizations engaged in a "socialist reconstruction of science." The theorists and architects of the five-year plans narrowed conservation to utilitarian positions with no room for ethical and aesthetic values. The emergence of "left deviationism" accentuated and aggravated the disunity in the house of Marxist doctrinaires. Despite the compounded difficulties, the conservation movement gained ground on one front: the central government showed ample evidence of its inclination to make *zapovedniki* not only observation outposts but also working laboratories engaged in scientific experiments. The new light, however, was only a screen for the onset of a dark age, heralded by the growing affiliation of official conservation policies with the pseudo-science of T. D. Lysenko. Weiner's discussion of the activities of I. I. Prezent, the philosopher and harbinger of Lysenkoism, provides an excellent introduction to the tragic era of "creative Darwinism," built on a theory of organic evolution that had no room for the achievements and experimental methods of modern genetics. Lysenkoism was much more an expression of a strong faith in the limitless powers of human beings to manipulate and exploit nature than a

recognition of critical needs for scientifically founded and vigorously enforced conservation policies. Thoroughly documented and firmly integrated, this book is a major contribution to the history of Soviet science, politics, and culture. Let us hope that Weiner's next study will cover the political and cultural dynamics of Soviet conservation during the post-1935 period.

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KLAUS SEGBERS. *Die Sowjetunion im Zweiten Weltkrieg: Die Mobilisierung von Verwaltung, Wirtschaft und Gesellschaft im "Grossen Vaterländischen Krieg" 1941–1943*. (Studien zur Zeitgeschichte, number 32.) Munich: R. Oldenbourg. 1987. Pp. 314. DM 68.

The Soviet Union's mobilization against the German attack of 1941 was highly surprising. Despite backwardness, ill-preparedness, and the sheer power of the Blitzkrieg, the Soviets stemmed the German tide within a few months, and one knows what resulted. How was this response possible? Stalin himself, early in 1946, gave a qualified push to a "miracle" explanation with his famous references to Marxian vision and party leadership. In the classic Soviet work on the war economy, published late in 1947, N. A. Voznesensky spelled this explanation out: he asserted that the State Planning Office (Gosplan), inspired by the party's socialist intuitions, had been able to manipulate economic laws. Later authors in both the Soviet Union and the West have invoked other factors also—Brother Frost, for example, Father Stalin, Mother Russia, or even Uncle Sam (that is, Lend Lease). But, not least because of the continued paucity of reliable data, the "miraculous" character of the Soviet mobilization, 1941–43, has remained largely unchallenged—whence, the great value of the present volume.

Klaus Segbers modestly abjures at the start any high revisionist aim, but what he does, in effect, is bring to bear on 1941 the sociological tools that revisionist historians have recently been applying to other periods in the development of Stalinism. These tools emphasize impulses from below and deprecate the "totalitarian" power up above. Segbers opens with a consideration of how the Soviet power system responded to the German assault and right away confronts head-on the Stalin-Voznesensky theses. He recognizes that it was simply not the Communist party that took the lead but a "statist" conglomerate of centralized, but "personalized" and "flexibilized" (p. 71), agencies under the State Defense Committee (GOKO). In effect a new bureaucratic structure simply by-passed the pre-World War II party-dominated administration. And then, dealing with the Gosplan, Segbers avers that, although it was central to the wartime economic administration and although it cooked up short-term "plans" during the war years, these were ad hoc, "operative" documents and constituted a sort of "decisionism" (p. 81) rather than "planning." He claims that it was not the

prewar frameworks but the newly found flexibility that helped the Soviet success.

The central portion of the book deals with the most notorious of the "miracles": the eastward evacuation of fifteen hundred factories and more than 10 million people from the western regions that the Germans occupied during the first two years of the war. Segbers breaks down the problem by discussing in each case first evacuation, then transport, and, third, reintegration. Rather rapidly he thus brings to light the essential issues. Above all, he shows that, although the Soviet government in no way directly oversaw everything that happened, it did from the start fight explicitly to rescue factories and workers. The Soviets were quite unlike the Germans in this respect, and, with public goals thus clear, the stage was set for local initiatives—and the appallingly clumsy transport system—to do the rest. And then Segbers shows that the miracles were relative: the fifteen-hundred-odd factories evacuated east came from a region containing thirty-two thousand industrial enterprises before the war. The 10 million people who got out were the residual of some 17 million who started on the trek—and remember those who stayed behind.

The final, and in many ways the most interesting, of Segbers's chapters concerns the changing character of the work force and of the Soviet national productive base during the first years of the war. Even in 1941 there was an enormous flow of male workers to the front and a correspondingly massive influx of untrained village recruits from eastern Russia and Siberia, most of them women and young people. Although there was some recall of pensioned older workers and some reliance on various forms of forced labor, the result was a huge retreat from skilled labor traditions. And this forced, or made possible, the other great change in the wartime Soviet workshop: a sharp increase in assembly-line production methods—in other words, a degree of modernization.

All of this is absorbingly interesting, and Segbers has very largely increased our understanding of a key area of twentieth-century European social and economic history. But one may question his organization of the material. Segbers sticks to Voznesensky's organization, whereby the "miracles" of the evacuation come first and overshadow the whole story of the Soviet war effort. But was it the fifteen hundred evacuated factories and the 10 million refugees who won the war after their resettlement in 1942 and 1943? Or was it the Urals arsenal, newly built just before the war, which doubled in size during 1941–43 even before it received the influx of enterprises from the West? I would guess that future students of the Soviet war economy might want to begin as well as end their story with the development of the east and that they might want to put the "miracles" permanently in a more humble place.

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ANTHONY D'AGOSTINO. *Soviet Succession Struggles: Kremlinology and the Russian Question from Lenin to Gorbachev*. Boston: Allen and Unwin. 1988. Pp. xvi, 274.

Heirs to Byzantine-Mongol-Muscovite despotism, the Bolsheviks never established a clear procedure of succession, and their failure produced half a dozen major state crises. With three just barely behind us, Anthony D'Agostino's pioneering attempt to explain a major component of what he calls the "Russian question" is especially timely.

D'Agostino dissects a stunning variety of Soviet émigré and Western literature on the succession problem, which he analyzes from an unusual angle, that of Comintern politics. In so doing he tends to place a greater burden on Boris Nikolaevsky and Boris Souvarine—to take just two prominent examples—than either man's political acumen can bear, and some readers will wonder just what the bit-players Lucien Laurat, Albert Treint, and Robert Louzon really have to do with Kremlinology, the succession question, or anything else Russian and Soviet. Nevertheless, as a kind of side excursion, D'Agostino conducts us on an informative tour of that Comintern puzzle whose solution still eludes scholars.

D'Agostino attempts to coin a term, "alternance," to describe what he maintains has been an incessant swing between rival Moscow and Leningrad "platforms." The Moscow platform urges "solicitude for agriculture, assurance for the denizens of the state machine . . . a policy of foreign participation in the economy, and détente in foreign policy" (p. 90); its authors were N. I. Bukharin, M. P. Tomskii, G. E. Zinov'ev, L. D. Trotskii, A. I. Rykov, and G. Ia. Sokol'nikov. On the other hand, the "Leningrad platform," which D'Agostino finds closer to original Bolshevism-Leninism, emphasizes heavy industry and the role of central planning, demands autarky, and insists on fomenting world revolution. Zinov'ev and Trotskii, along with E. A. Preobrazhenskii, count as founders of this policy.

It would seem that "alternance" would bring now one, now the other, line to prominence. Not so. D'Agostino argues that the center has always held in Soviet politics and that both domestic and foreign policy has reflected careful tacking by leaders seeking to stay in the mainstream.

No one has yet squared this well-known view with the events of 1928–53. D'Agostino, however, states it in the context of the Comintern, thus casting a different and intellectually stimulating light. In so doing he unwisely seeks to rehabilitate political dunces such as Zinov'ev far beyond the juridical vindication awarded them—posthumously to be sure—under *glasnost'*. Zinov'ev emerges here as a major thinker, yet D'Agostino acknowledges that he was "rarely consistent in anything" (p. 4) and observes that "Trotsky demonstrated that, in sheer obfuscation, he could compete on a level with Zinoviev" (p. 80).

D'Agostino's book has some flaws and would have benefited from serious editing. But it is the work of a

bold, knowledgeable scholar who has dared to venture into imperfectly charted waters, and students of Soviet and Comintern politics will find much of real value in it.

WOODFORD MCCLELLAN
University of Virginia

MOSHE LEWIN. *The Gorbachev Phenomenon: A Historical Interpretation*. Berkeley and Los Angeles: University of California Press. 1988. Pp. xii, 176.

Moshe Lewin's work is known to every student of Soviet history. His studies of the decision to collectivize agriculture and its impact on the party and society helped give a push to the recent work of many in the social history of the Stalin period. His study of the struggle between Lenin and Stalin in 1922–23 provided a superb point of departure for interpreting the succession to Lenin and, in consequence, the whole pattern of ideological struggle in the party to this day. So who better than he to provide historical perspective on Mikhail Gorbachev's reforms? This book, an exercise in what Lewin calls the "history of the present," thus attempts to tell us through what stage Soviet society is currently passing.

Lewin judges that the Soviets are once again trying to transcend the "bi-model pendulum" (p. 152), an alternance between a relatively loose economic and social regime, such as the New Economic Policy of the 1920s, and the military-bureaucratic siege mentality of the War Communism period, 1918–21. Escape from that alternance would mean the dawning of "an age of political reason." Such an escape was unsuccessfully attempted during the 1960s by a coalition of economists and managers with the support of Prime Minister Alexei Kosygin. That reform is once again being tried is evidence that Soviet society did not stagnate during the Brezhnev years along with the party and state apparatus but has continued to develop to the point where its old relation to a state that tried to oversee every phase of public and private life is no longer adequate.

Lewin has long been at odds with more traditional interpretations that have seen totalitarianism as having been fixed on Soviet society as a more or less permanent consequence of the events of 1917. Instead, he has always insisted that economic and social backwardness provided the essential preconditions for Stalinism. The "archaization" (p. 16) of agrarian life and production, caused by the peasant war of the revolutionary period, was the central problem that Soviet Communism faced in the 1920s, a problem that it solved by the Stalinist "big drive" of the 1930s. The system established at that time, Lewin argues in a tone comparable to Soviet reformers such as Tatiana Zaslavskia, is now outpaced by its victories. Soviet society has become an urban one with a professionalized work force and a better-educated intelligentsia. Even under Stalin, society was never completely prevented from expressing its little

resistances, and now those have developed to the point of constituting a genuine public opinion. Moreover, the intelligentsia is becoming aware of its new role by virtue of the work of academic social scientists, whose studies of the social changes are pressing on the state a recognition of the need to adjust. It is the energy of "civil society recovering" (p. viii), therefore, that has produced Gorbachev.

Lewin's analysis may recall the expectations of the immediate post-Stalin period of the 1950s, when Nikita Khrushchev appeared to be borne up on a wave of Soviet military victory and economic promise, boasting of Soviet technical prowess and vowing to make good the industrial lag behind the West in short order. Observers such as the Stalin biographer Isaac Deutscher at first thought that the post-Stalin circumstances would prompt a renunciation of Stalinism root and branch. Then they made progress conditional on the elimination of the industrial and technical lag in a "great contest" with the West. They were less wrong than often supposed, but, by the early 1960s, Soviet Russia had again demonstrated the fallacies of hope. Lewin, however, is not stoking up such grand expectations for the second wave of reform. He thinks it reasonable to foresee only a "new authoritarianism" within the framework of a one-party system, perhaps beginning simply with "an active, vigilant, and aggressive public writing letters and articles, but also joking, gossiping, and grumbling" (p. 131).

It might only be added that the Soviet state shown us by experience has not only a capacity to adjust to new social conditions but also what the liberal historian of tsarist Russia Paul Miliukov called a "critical" faculty, a will to shape and rectify social conditions. Resistance to *perestroika* is not simply a matter of "conservatism" and bureaucratic sluggishness; it is also a mobilization of a part of the revolutionary heritage, however distorted, for a struggle against "opportunism." This is a reminder that alongside the adjustment to modern conditions, there must be victories in the realm of ideas, a reinterpretation of experience, a sense of renewal. Sociologists such as Zaslavskaja are always open to the charge of smuggling in Western bourgeois social science, a charge that Lenin once hurled at Nikolai Bukharin. It would be surprising to find Soviet sociology supplanting official ideology by increments without a sharp and prolonged confrontation with the official doctrine.

That Soviet political life has generated similar expectations during previous "thaws" cannot be taken to mean that the Soviet Union cannot change but rather that the passing of time repeatedly forces the question of change back onto the agenda. The reader can consult no better demonstration of the eligibility and preparation of Soviet institutions for change than Lewin's fine study.

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NEAR EAST

PATRICIA CRONE. *Roman, Provincial, and Islamic Law: The Origins of the Islamic Patronate*. (Cambridge Studies in Islamic Civilization.) New York: Cambridge University Press. 1987. Pp. 178. \$39.50.

Readers of Patricia Crone's previous books will find little in this work to surprise them. It is erudite, very lucid (considering the topic), and logically arranged. Along the way, Crone positively drubs her reader with footnotes, argument and source criticism, and, of course, *terminae technicae* in Latin and Arabic; as a result it is sometimes a little difficult for the reader to keep track of the shape of both her argument and her evidence. Unfortunately, the work also continues her peculiar nineteenth-century quest for the "true (foreign) origins" of classical Muslim institutions.

Clientage (*walā'*) is a kind of adoption whereby in pre-Islamic times a person with no prior connection to an Arab tribe became a member and the client of a specific member of that tribe. The client had certain responsibilities vis-à-vis his patron, and the patron enjoyed certain privileges such as that of inheritance or the right to a percentage of the client's earnings for a period that extended sometimes until the death of either patron or client. Much has been made of clientage as the "traditional" means by which foreigners became de facto Arabs, and it has been assumed that the Arabs, after the conquests of the 630s and 640s C.E., simply adopted this institution to integrate non-Arab Muslims into the Arab empire, an approach that grew more and more problematic until the 'Abbāsid revolution of 750 C.E. when conversion became a means for full integration into the Muslim state.

Crone argues here that clientage in its classical Islamic form is discontinuous with the pre-Islamic Arab institution of the same name. Instead, Crone proposes that there are three distinct stages in the history of the institution. In reverse order they are the classical Islamic stage (roughly 750 C.E. onward)—clientage as we know it from the texts; the Umayyid phase (660–750)—clientage in the first days of Arab dominance of the Near East; and, disconnected from its successors, the pre-Islamic Arab form of clientage (to 622?). Crone's point is that the "source" of the classical and Umayyid institution is not Arab custom but something called "Roman provincial law."

Since it is so important to her enterprise that Islamic institutions be neither Arab nor Muslim in origin, she wants to be able to point to Roman provincial law as an institution of substance to which Muslims had recourse when they needed more complex rules of organization than those they brought with them from the Hijaz. It appears, however, that what Crone grandly styles "Roman provincial law" was in large part, particularly for rules of clientage, Near Eastern social custom, enforceable in court. Roman provincial law was not imperial law; it was largely not codified, as I gather from her references; it is not even clear in what sense Roman provincial law was Roman, since aspects of Roman

provincial law of clientage are found as far back as Hammurabi.

So the picture that emerges from Crone's study is this: Muslims inherited prevailing customs and in some cases garbed them in *hadith* or fictitious pre-Islamic practice. The word "inherited" is, of course, not her word of choice; it does not convey the requisite sense of dependency, derivativeness, and deficiency. It is, however, the proper term since the various groups in the Eastern Mediterranean of the seventh century did not live in ethnic and religious vacuums but belonged to a common culture and were ruled by common institutions. That Muslims inherited indigenous institutions makes perfect sense. Only the most pious could object. But Crone's agenda is to show the abjectly dependent nature of Islamic law, and in this she fails. Through her scrupulous investigation, what she in fact documents (but does not argue) is the creativity of an Islamic culture that felt free to borrow but also to discard and that both integrated itself into and distinguished itself from its predecessors.

This is a slight but meticulous book, and anyone concerned with the social history of the early Islamic Near East should read it. As good scholarship sometimes does, however, it succeeds precisely by subverting its own argument.

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YEHOShUA BEN-ARIEH. *Jerusalem in the Nineteenth Century: Emergence of the New City*. New York: St. Martin's or Yad Ishad Ben-Zvi, Jerusalem. 1986. Pp. xiii, 509. \$29.95.

In this second volume on the historical geography of Jerusalem in the nineteenth and early twentieth centuries, Yehoshua Ben-Arieh traces the growth and development of the New City of Jerusalem, outside the walls of the Old City. In the first volume, the city within the walls is described as a backward, overcrowded, and dilapidated Oriental city, whose geographic patterns were determined by the location of the principal monuments of the three major religions: Judaism, Christianity, and Islam. In contrast, the New City is characterized by its modernity, which the author associates with cleanliness, well-ordered houses and neighborhoods, and rapid development. Above all, and here lies the central thesis of the book, the New City has a basic Jewish character (although Christian and Muslim developments outside the walls are also discussed). The predominately Jewish new sector grew into a city of neighborhoods rather than developing around religious monuments as did the Old City. Jewish settlers, often from a common community or with a common ethnic origin, constructed whole neighborhoods by forming societies or associations. Toward the end of the nineteenth century, commercially built neighborhoods became the standard.

Based almost entirely on travelogues, memoirs, and

newspapers from the Hebrew press, the history of the New City is reconstructed neighborhood by neighborhood (and sometimes house by house), by using extensive quotations from contemporary observers, old photographs, and maps. There is little analysis of the lengthy and sometimes contradictory quotations that link the narrative together, but each chapter and sometimes subsection is concluded with a short summary of the main features that the sources describe. It is only at the very end of the book that the author attempts to paint a general picture of the New City, although with little concern about what kind of urban typology this experience might suggest. The growth of a modern sector outside the ramparts of the older "traditional" city was a process going on elsewhere in the Middle East, yet the author makes no reference to other urban studies that might have helped elucidate some of the processes occurring in Jerusalem. Ben-Arieh points out that the New City was formed by Jerusalem's indigenous population (the old settlement or *Yishuv*), only later joined by newcomers. Yet the emphasis of the book is not on the links between the old and the new sections of the city but on the essentially different character of the New City. One is left with a somewhat truncated analysis, with little sense of how the city's markets and thoroughfares were tied together, or how the many communities and ethnic groups that made up the city as a whole were connected.

At the same time, the book does have some important things to say about the place of Jerusalem in the growth of Jewish settlement in Palestine. Jerusalem's relationship with Zionist settlers of the new *Yishuv* was not very harmonious. The Eastern European settlers of the First and Second Aliyah looked disparagingly toward Jerusalem and its more traditional and religious Jewish inhabitants who still lived, to a large measure, from the charitable contributions (*halukkah* funds) sent by the Diaspora. The Eastern European Zionists had relatively less of an impact on the growth of Jerusalem than the Bukharan, Persian, Kurdish, Yemenite, North African, and other Oriental Jewish communities that began to form new neighborhoods at the end of the nineteenth century. The settlement of Oriental Jews in Palestine has been largely neglected in Zionist historiography until relatively recently, and Ben-Arieh's study contains important details on this question. The two volumes now available in English provide the reader with invaluable details and innumerable bibliographical references, inviting scholars to interpret what Jerusalem's modern history can tell us about the urban experience.

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ABDEL-RAOUF SINNO. *Deutsche Interessen in Syrien und Palästina 1841-1898*. (Studien zum modernen Islami-

schen Orient, number 3.) Berlin: Baalbek. 1982. Pp. xix, 437. DM 36.

Following the Egyptian occupation (1832–40) of those parts of the Ottoman empire commonly referred to as Syria and Palestine, European and American missionary activity, commercial and political interests, and scientific and Biblical studies in the area increased. Recent monographs have focused on American, British, Russian, Austrian, and French involvement in the region in the mid- to late nineteenth century but have ignored or viewed tangentially the interest of the German states prior to confederation and, subsequently, the German empire. Although Gad Bensinger in his dissertation, "Palestine in German Thought and Action, 1871–1914" (1971), considered aspects of German activity in the region, an incisive study remained to be undertaken.

Setting out to fill this void, Abdel-Raouf Sinno has given the most detailed look at the subject to date but has failed to analyze it fully and has virtually omitted consideration of the activities of the *Templerkolonien*, and German archaeologists and exploratory associations, as well as of the German relationship to Jewish aspirations in Palestine.

Sinno concludes that Germans had neither extensive interests nor unqualified successes in Syria and Palestine in the nineteenth century. Between 1841 and 1898, no systematic German policy emerged to transform the region into a German sphere of influence. Momentum for a German presence, especially prior to 1880, came primarily from German religious groups, to which two-thirds of this volume is devoted. Yet Sinno suggests that the extent of the work undertaken by those groups benefited from the willingness of the German state(s) and public to help prepare the way and to provide backing.

The book commences with a somewhat limited study of the development of the Anglo-Prussian bishopric in Jerusalem, which lasted from 1841 to 1886. Sinno then proceeds with a detailed examination of the German Protestant missionary endeavors in Palestine and Syria that followed the bishopric's establishment. One chapter treats the more limited efforts of German Catholic organizations. The author offers copious information about German-run hospitals, schools, a leper colony, hospices, agricultural communities, vocational training programs, and orphanages, including their finances and organization. By considering each missionary enterprise individually, in often overwhelming detail, Sinno's study becomes ponderous, loses analytical depth, and obscures overall relationships. Clearly, conversions were sparse. The missionaries' lasting legacy became those charitable works that received Ottoman approval. Sinno believes that those works helped prepare the way for subsequent strengthening of German economic and diplomatic ties with the Ottoman empire.

The final third of this book addresses German commercial, diplomatic, and colonial interests in the re-

gion, which increased after 1880. Presented somewhat chronologically, the discussion here is more thoughtful. The relationship between German trade with Syria and Palestine and internal German economic conditions, diplomatic considerations, colonial interests, and the policies of Otto von Bismarck and William II is presented. Sinno leaves no doubt that, although German trade with the region was expanding, it was of no real consequence to the German empire, accounting for only 0.2 percent of its foreign trade by 1900. The work concludes with the visit of William II to Palestine and Syria in 1898, which is viewed as significant because it encouraged German economic concessions elsewhere in the sultan's domain.

Sinno leans heavily on published annual reports, periodicals, and special histories produced by the various religiously oriented organizations. He makes occasional reference to Prussian, imperial German, and British unpublished diplomatic documents. With regard to trade statistics, Sinno ignores the useful published British commercial, consular, and diplomatic reports and official statistical abstracts. A map and a full subject index, not one limited to the names of individuals, would have enhanced the book.

ELVA BOGERT CRAWFORD
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TIMOTHY MITCHELL. *Colonising Egypt*. (Cambridge Middle East Library.) New York: Cambridge University Press. 1988. Pp. x, 218. \$42.50.

The study of the British colonization of Egypt has been a developed field of scholarship for a generation. It contains Marxist studies of imperialism and "Commonwealth School" studies defending the motivations of the leading European politicians of the time. Recent neo-Marxist scholarship, to which this work is partly indebted, suggests a possible synthesis of these approaches. For example, Alexander Scholch found that the catalyst of the British decision to invade Egypt emerged as the brainchild of Europeans in Egypt whose interests were threatened by the rise of the nationalist Ahmad 'Urabi (Scholch, "The 'Men on the Spot' and the English Occupation of Egypt in 1882," *Historical Journal* 13 [1976]: 773–86). They frightened the Foreign Office with their reports and manipulated London into undertaking the invasion. Overall, the interpretation of the colonization of Egypt has been moving in recent years in the direction of events in Egypt and away from events in London. The works of Egyptian social historians, such as those of 'Ali Barakat and 'Asim al-Dissuqi, appear to define the modern discourse by outlining the broad contours of Egypt in the nineteenth and early twentieth centuries. The virtue of the present book is to find a fresh angle of inquiry through poststructuralist literary criticism.

The originality of Timothy Mitchell's work lies in two areas: the European cultural preparation for colonial

hegemony lasting throughout the nineteenth century and the role of the Egyptian elite in this preparation. If, as Mitchell argues, colonialists produce a new notion of self and "other," he should then deconstruct the history of museum exhibits, world's fairs, photography, and orientalist conferences as Edward Said did novels. In addition, the indigenous elite in Egypt must have played a role in validating new representations of themselves and their country. This they did, even if they were critical of some European stereotypes in point of detail. Mitchell includes the views of influential Egyptians, who happened to witness the representations of their own society. He shows how the ruling class, on encountering its identity in the European mind, found it strategic to Europeanize itself and to remove the features that made it an "other" for Europeans. To do this rulers were prepared to agree that much of Egypt was a barbarous mystery in need of the modernity and rationality only to be found in any large measure in Europe.

Mitchell studies the career and writings of a "newly de-traditionalized" elite intellectual, the scholar and engineer Ali Mubarak, and a "neotraditional" scholar, Husayn al-Marsafi. After Mubarak encountered Europe, he returned to Cairo and set about westernizing the street system and the education system. The Foucauldian approach serves well in explaining the connections between Mubarak's seemingly unrelated enterprises; a plan to change the organization of the urban design of Cairo led to the creation of open spaces and regular streets yet was also related to the plan to change the minds and habits of students. Mitchell also delves into the way in which the strategies employed by the Europeanizing elite were rationalized in an Islamic idiom. The Egyptian government presented its new demands to the peasantry not simply by coercive measures but as part of Islamic tradition. Mitchell demonstrates that tradition with an original reading of the famous essay on eight words by Shaykh Husayn al-Marsafi, a religious intellectual, whose family owned land in the Delta. Mitchell's reading emphasizes the logic of adjacency; Marsafi's logic is not simply a vertical part of Islamic tradition but also a horizontal part linked to that of Lord Cromer, whose *Modern Egypt* (1978) conveys a hope of creating similar bonds between ruler and ruled. A shared logic of adjacency explains other apparently unrelated phenomena as well, for example, the feminism of Qasim Amin.

Mitchell appears to have constructed this book to stimulate two audiences: a small one interested in mid-nineteenth-century Egypt and a larger but less cohesive one interested in cultural representations but known to prefer the Middle East as a fantasy world. It will be the second that will find Mitchell's work provocative.

PETER GRAN
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JOHN CHARMLEY. *Lord Lloyd and the Decline of the British Empire*. New York: St. Martin's. 1987. Pp. x, 294. \$29.95

John Charmley's book proves that old-style panegyric biographies are not a thing of the past. Following Charmley's first book, *Duff Cooper: The Authorized Biography* (1986), this biography has all of the hallmarks of another "authorized" study. This is not, however, to discount Charmley's extensive research. He has made good use of primary sources and has clearly enjoyed extensive access to the personal papers and reminiscences of the Lloyd family. Primary source material and collections of papers have been usefully arranged in the bibliography by location rather than by the more commonly used alphabetical order.

Charmley does a creditable job in the descriptions of Lord Lloyd's rather checkered career as a Tory politician; the account of Lloyd's role in the Conservative party and of party policies prior to and during World War II is particularly informative. The author is clearly at ease with this material and evidently has had considerable "in-side" information concerning Winston Churchill and his coterie.

Unfortunately, most of Lloyd's career was spent not in party politics but in ranking diplomatic positions in India and Egypt. That Asia and the Middle East are not Charmley's area of specialization is evident on a number of points. There are relatively minor slips, such as references to the Witantist for Hizb al-Watani (National party) or to Abbas Himli for the former khedive Abbas Hilmi. More discomfiting is Charmley's uncritical acceptance of Lloyd's hard-line imperialist and sometimes openly racist viewpoints. On this point, one example out of many will suffice. Lloyd's opinion about "the fundamental unsuitability of modern western democratic methods of government to any Oriental people" (p. 170) passes without comment. Such a sweeping generality might well bemuse, if not outrage, the Indian people who, in spite of formidable obstacles, have preserved the world's largest democracy for over forty years.

Charmley attempts to soften the general perception of Lloyd as an aloof and authoritarian personality by recounting sometimes charming anecdotes about Lloyd's devotion to friends and family. The overwhelming majority of accounts regarding Lloyd's demeanor as a public representative of the British empire indicate, however, that he was both overbearing and autocratic. Charmley chooses to discount Egyptian sources on this point, nor do his sources include the comments by J. Morton Howell, the U.S. ambassador to Egypt during part of Lloyd's appointment to Cairo. Howell's *Egypt's Past, Present and Future* might be disregarded on the grounds of his well-known Anglophobia, but such can hardly be the case for similar comments by numerous British officials in Cairo and London. For example, in his unpublished diary (not cited in the bibliography), which is part of the collection of St. Antony's College, Oxford, Owen Tweedy, a British

official in Cairo, fretted that Lloyd wanted to annex Egypt: "I hope I am wrong but again and again the word . . . crops up without anyone lowering his voice."

In summary, Charmley justifies Lloyd's fervent support of empire and seemingly agrees with the dire predictions Lloyd made regarding future calamities if the empire collapsed. Although some may still mourn the death of the British empire, perhaps an American minister to Cairo might have the last word. In an unpublished memorandum to the State Department from Cairo in 1929, Franklin Mott Gunther observed that Lloyd was "a dyed in the wool imperialist and really, in this century, an anachronism."

JANICE J. TERRY
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MUHAMMAD Y. MUSLIH. *The Origins of Palestinian Nationalism*. (Institute for Palestine Studies Series.) New York: Columbia University Press. 1988. Pp. xiii, 277. \$30.00.

The precise nature of Palestinian Arab nationalism has been elusive. Most studies suggest, with some ambiguity, that because of Zionism it has been more Palestinian than Arab. Muhammad Y. Muslih believes that Zionism has been overemphasized and the social and political milieu neglected. He offers a new explanation on the basis of a study of "the political elites of Palestine and their ideology from 1856 until December 1920, when the third Palestinian Arab Congress was held in Haifa to decide the future of Palestine" (p. ix). During this period the dominant actors in Palestinian politics were the urban notable families.

In the second half of the nineteenth century, the notables enhanced their economic and political position and became an aristocracy of service. This elite subscribed to the dominant ideology of Ottomanism, but there was intense competition for place among its members. After 1908, unsuccessful members turned to Arabism in reaction to Young Turk policies, but Ottomanist notables who held office remained dominant until 1918. Meanwhile, by 1914, anti-Zionism had become a widespread movement in Palestine.

The Ottoman collapse in World War I, the Arab government under Faysal in Damascus, and the British military administration in Palestine negated Ottomanism, so Arabism was adopted by the older Ottomanist notables to protect their position from their Arab nationalist rivals, now strengthened by younger converts, who could look to Faysal's government for support. Consequently, the two years following the end of the war were filled with a conflict between the older politicians, the erstwhile dominant Ottomanist notables, and the younger politicians—the older group organized in the Muslim-Christian Association, the younger in the Literary Club and the Arab Club. The two factions shared as the major objective the defeat of the Zionist program, but the notables' preference was for an independent Arab Palestine while the younger group favored the inclusion of Palestine in an indepen-

dent Greater Syria. The younger politicians, using their connections with Faysal and the Syrian and Iraqi Arab nationalists, carried the day early on, so the Palestinians joined the Syrians in proclaiming the independence of a united Syria under Faysal.

The appeal of Arab unity was countered by personal and local interests. Palestinian, Syrian, and Iraqi notables regarded each other as alien would-be usurpers. Local interests took first place. The Palestinians, concentrating on the Zionist danger, blocked compromise with the British and the Zionists to secure a French-free Syria and with the French to secure an Arab Syria under French mandate. The consequent imposed partition only hastened the already existing fragmentation along provincial lines. Thus, the Palestinians abandoned union with Syria for Palestinian independence. So Zionism contributed to the formation of a separate Palestinian Arab nationalist movement, but the interplay of internal interests and the policies of Britain and France would have created such a movement in any event, as those factors did in Syria and Iraq. Even so, the Palestinians never abandoned the idea of Arab unity and, in fact, failed to create a term for Palestinian nationalism.

The author's exposition is sometimes ambiguous to me, but the main points seem clear. The purported influences of Young Turk nationalism and of a Palestinian generational cleavage are dubious, but generally the discussion is persuasive. The research is admirable. Besides the British and Zionist archives, a substantial quantity of important Arab private papers, some published but most unpublished, and many interviews have been used. The new sources do not indicate any major revisions of previous studies with respect to particulars, but they do increase reliability and greatly extend and improve comprehension. In short, the book is an important advance in modern Arab historiography.

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BENNY MORRIS. *The Birth of the Palestinian Refugee Problem, 1947–1949*. (Cambridge Middle East Library.) New York: Cambridge University Press. 1987. Pp. xx, 380. \$39.50.

This important, meticulously researched work is the first to deal in a scholarly fashion with the problem of the Palestinian refugees. Benny Morris treats in great detail the Arab exodus during the war of 1948 and more briefly the diplomatic efforts to resolve the resultant refugee crisis. His major conclusion, based on a village-by-village study, is that "the Palestinian refugee problem was born of war, not by design, Jewish or Arab. It was largely a by-product of Arab and Jewish fears and of the protracted, bitter fighting that characterised the first Arab-Israeli war; in smaller part, it was the deliberate creation of Jewish and Arab military commanders and politicians" (p. 286).

There never was any Israeli political or military plan to expel the Arabs. Once the exodus began, however, the opportunity to reduce Israel's Arab minority to manageable proportions was simply too fortuitous for the government to forego. Military logic and political strategy dictated the exploitation and perpetuation of the Palestinians' precipitant flight.

Morris could have taken more space to outline the history of Arab-Jewish relations in Palestine during the thirty years of the mandate, especially during and after World War II. It is difficult now to evoke, or even comprehend, the emotions and apprehensions of the Yishuv (the Jewish community in Palestine) on the eve of its war of independence. Pogroms in Palestine during the mandate, and the Holocaust, had convinced the Israelis (and many foreign diplomats) that, if they lost, they might expect to be slaughtered.

The Arab flight began during the first phase of the war, from December 1947 to March 1948, when the Palestinians in fact held the upper hand, in the "battle of the roads." Many of the Arab upper and middle classes fled, especially from Haifa and Jaffa, towns destined to be inside or at the mercy of the Jewish state. The urban masses and fellahin obviously had fewer resources and were less mobile. But, as the British packed up, Arab morale plummeted, and services, businesses, schools, and clinics closed down as Arab leaders abandoned the civil service and municipalities. The Arabs could not match the Yishuv in its communal discipline and institutions.

The second phase of the Arabs' flight began in April 1948, when the Hagana went on the offensive. Its strategic objective was to secure control of the roads connecting Jewish towns and settlements. From a purely military point of view, this required clearing arterial roads and border areas of potentially hostile Arab communities. Thus, military commanders could expel Arabs from militarily sensitive areas. Even so, expulsions were determined by local circumstances and commanders, who were influenced also by the anticipated invasion of five Arab states on May 15.

As the Hagana emerged supreme, Arab morale cracked, and the Arab exodus became a panic-stricken flight. There was also the "atrocities factor" (the Deir Yassin massacre in early April), which Arab propaganda embellished and magnified, thereby increasing Arab panic. In contrast, Arab atrocities (the retaliatory massacre in April of Jewish doctors and nurses headed for the Hadassa hospital in Jerusalem) did not demoralize the Jews or precipitate their mass flight. Morris estimates that two dozen villages were ordered to evacuate by the Hagana. Arab commanders also ordered the "temporary" evacuation of a similar number of Arab villages for military and political reasons—the desire to clear the battlefield or fear that villages would acquiesce in Israeli rule. During the third phase, the war against the Arab states in July, October, and December 1948, an additional three hundred thousand Arabs left. Many fled because of Israeli attacks or fear of them, yet months of internal collapse and demoral-

ization had already prepared Arab communities for the move.

The fighting in July was preceded by a specific Israeli general staff order not to destroy Arab villages or expel inhabitants without the authority of the defense minister, David Ben-Gurion. He never issued such orders, appreciating well the moral and historical ramifications and fearing the sanctions of the international community. But the unanticipated opportunity presented by the Arabs' own initial flight was obvious to and "understood" by all. Once the Arabs had left, their return was blocked—by the Arab states' insistence that the refugees' return be the first step in any peace settlement; by the Israelis' insistence that, on security grounds, only a partial return would be allowed, as part of an overall peace settlement; and by the physical destruction of some villages and the takeover of Arab property by neighboring Jewish settlements and new immigrants.

There is a generic problem with the source material for such a book. Many Israeli sources remain closed, and much of the available material is subjective, even selective personal memoir. This situation has led Morris to many assumptions of probability and some questionable conclusions. No Arab country has opened its archives, and secondary Arab material has to be treated with the same circumspection as Israeli accounts. Morris's claim to have used "some contemporary Arab memoirs and diaries" (p. 2) appears misleading, since the author does not apparently read Arabic. Finally, a word of praise for the courage of the author, who has presented us with a scholarly, if not definitive, study, and for a society that permits its citizens the freedom to deal so openly and frankly with such a sensitive and controversial subject.

MICHAEL J. COHEN
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AVI SHLAIM. *Collusion across the Jordan: King Abdullah, the Zionist Movement, and the Partition of Palestine*. New York: Columbia University Press. 1988. Pp. x, 676. \$40.00.

Avi Shlaim has written an important book. He brings new information gleaned from Israeli sources to the topic of Israeli-Jordanian and British-Jordanian cooperation in the 1948 war and in Jordan's subsequent annexation of the West Bank. His revisionist thesis is that an agreement in 1947 between King Abdullah of Jordan and representatives of the Jewish Agency to carve up Palestine between them laid the basis for continuing collusion during and after the war. Subsidiary to that is a second thesis that Britain secretly endorsed Abdullah's ambitions to add part of Palestine to his kingdom.

That the subsidiary thesis is certainly true has been amply demonstrated by other recent books (for example, William Roger Louis's prize-winning *The British Empire in the Middle East, 1945–1951* [1984]), which discuss the Anglo-Jordanian understanding from the

point of view of British and Jordanian self-interest. Shlaim's focus is on Israel's misreading, at the time and until today, of Britain's intentions regarding the emerging Jewish state. He concludes his assessment of British-Jordanian collusion with a refreshing observation: "Thus Bevin, who is usually portrayed in Zionist accounts of this period as the great ogre who unleashed the Arab armies to strangle the Jewish state at birth, emerges from the documents as the guardian angel of the infant state" (p. 618).

The first and main part of Shlaim's thesis is less well sustained than is his subsidiary argument. He begins with the most complete description yet published in English of Golda Meir's meeting with Abdullah in November 1947. The understanding forged then was that the Zionists would agree to Abdullah's annexation of that part of Palestine to be allotted to the Arabs by the United Nations in return for his agreement not to attack Jewish forces or frustrate the establishment of a Jewish state. The vision of an easy and natural extension of Abdullah's rule into an uncontested or only nominally contested Arab Palestine was clouded, however, by Zionist aggression beginning in March 1948 into the Arab portion of Palestine designated by the UN. Thus, the understanding of November 1947 was broken by the Zionists.

The Arab Legion never did attack Jewish territory owing to the influence of its British officers, the understanding with Britain, and an appreciation of the Legion's limited capacities. It did engage Israeli forces but only in Arab territory and in the international enclave of Jerusalem. Shlaim's assumption that such engagement was a Jordanian violation of the bargain, but that Israeli aggression into territory promised to Abdullah was not, seems one-sided.

The collusion between Israel and Abdullah was therefore sporadic and occurred only when it suited Israel, which had preponderant force and international good will on its side. But, to maintain the possibility of cooperation, there were numerous meetings between Abdullah or his emissaries and Israeli representatives before, during, and after the war. Their conversations and what they inadvertently reveal of intent, understanding, misapprehension, and mutual interest make up the fascinating core of this book. At times mutual interests did make for complementary policies, as, for example, Jordan's quiescence during Israel's attacks on Egyptian forces in the Negev in October and December 1948. At other times cooperation with Jordan stood in the way of Israeli goals and was abandoned.

The great strength of the book lies in what Shlaim reveals from Israeli sources. His use of British sources is also exhaustive. His understanding, however, of the Arab side (Palestine, Jordan, and Egypt being the main players) is somewhat lacking, as is betrayed by his use of such phrases as "Oriental diplomacy" (p. 23). His introductory sketch of Jordanian history also has several errors of fact.

But these are minor quibbles in what is an important

addition to the growing body of work by Israeli scholars attacking the myths surrounding the founding of the state. Owing to the length and undigested manner of Shlaim's presentation, however, his corrective view of Israeli-Arab relations may not have the impact it should have.

MARY C. WILSON

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GEOFFREY JONES. *The History of the British Bank of the Middle East*. Volume 1, *Banking and Empire in Iran*; volume 2, *Banking and Oil*. Assisted by FRANCES BOSTOCK *et al.* (Hongkong Bank Group History Series.) New York: Cambridge University Press. 1986; 1987. Pp. xxiv, 418; xxi, 359. \$155.00 the set.

As part of the effort by the management of the Hongkong Bank Group to produce a serious history of their firm, Geoffrey Jones has written a detailed study of the British Bank of the Middle East, an institution founded in 1889 (as the Imperial Bank of Persia) to bring modern banking to what is now Iran. Volume 1 deals with the rocky path followed by the bank from its beginning to its withdrawal from Iran in 1952. Volume 2 traces the history of the bank's penetration of the Arab world (and of such subsidiary areas as India and Switzerland); the focus is on the years since World War II.

The research is exhaustive; the author provides a balanced history of the bank's triumphs and failures; and to a large extent he succeeds in giving the reader an understanding of both the technical (finance, labor relations, marketing) and the personal aspects of the bank's development. Moreover, Jones ventures into the largely ignored area of political economy in his attempt to analyze and assess the relationships that developed between the bank, the British government, and the leaders of the newly emerging Middle Eastern nations. Despite these successes, the reader is left with an unsatisfied feeling, which may, however, reflect more on the general problems inherent in writing business history than on this example of the genre.

A reader with an interest either in economic development or in nineteenth- and early twentieth-century imperialism will find volume 1 revealing and enjoyable. In the 1890s, Persia was almost completely undeveloped, and the author tells a story that is almost beyond belief of the bank's attempts to deal with the problems associated with creating a "national" bank. The difficulties inherent in coping with a political system that was despotic and chaotic and that operated largely without established rules were relatively minor when compared to the day-to-day issues that faced the new managers of the new bank. How does one control branches scattered over a large country, or, at an even more basic level, how does one transfer notes and reserves from one town to another when there is no transport system? How is it possible to operate profit-

ably in a country when there are neither funds for deposit nor loan opportunities in any industry or on any terms within the range of one's experience? How can one staff an enterprise when recruits can neither speak the language nor operate efficiently in the environment? Jones demonstrates fairly conclusively that one probably cannot, and those who operated the British Bank of the Middle East almost certainly did not.

Volume 2 will probably be of more interest to historians and political scientists who specialize in the modern Middle East. Jones tells the story of the shift in the bank's area of operation to a region that was initially precapitalistic (and, perhaps, even prefeudal), that, with the discovery of oil, became intensely capitalistic, and, in many cases, soon thereafter postcapitalistic and nationalist. It is the story of bankers faced with a rapidly changing political situation over which they had no control and trying, albeit ineffectively, to make the best of an impossible situation.

Finally, both volumes together will be read with interest by business historians concerned with explaining the factors that lead to business success, on the one hand, and failure, on the other. In this, the case of a major pioneer firm in a developing market, the question is not why did the bank ultimately fail, but how did any enterprise operated as incompetently as this one manage to hang on for almost a century. The men who ran the bank hated competition with a passion, but they proved hopelessly inept in their attempts to manipulate the political structure to smother potential competitors. They refused to employ "foreigners" in positions of responsibility, despite the fact that no one with the appropriate birthright had any understanding of local conditions in the Middle East. Moreover, while limiting senior recruiting to the British Isles, they refused to employ college graduates or pay competitive wages to the workers whom they did recruit. Finally, they made almost no attempt to develop an understanding of local problems or institutions (most of their employees did not even speak the local language). They preferred instead to impose British banking rules and regulations and, despite the problems of transport and communications, home office direction directly from London, of the most minute operating details. It is hardly surprising that, except when war made it possible to exploit the British or Allied governments or when a sudden surge of petroleum exploration and production made it possible to capitalize on the bank's temporary monopoly of place, the British Bank of the Middle East proved less than a rousing success.

Jones has produced a very useful work. It does, however, suffer from two weaknesses. In the first place, there is a lack of analysis based on quantitative data. Every student of nineteenth-century business recognizes that those firms pursued a variety of accounting methodologies, including "lie if you can, otherwise report nothing." But, with a minimum of assumptions and the application of a modicum of economic theory,

it is usually possible to reconstruct some useful series from what fragmentary records do exist. Moreover, in the present century, almost all firms have too many, rather than too few, records.

Thus, for example, if one assumes nothing more than the efficient market hypothesis—and the London capital market was efficient—it should be possible to construct an *ex post* investors' rate of return series by summing dividends and capital gains and dividing by the market price of the securities (in real terms since the time period was a lengthy one) and to use those results as estimates of the bank's intermediate and long-term profitability. Similarly, for the recent past, the study would have been more useful if the author had analyzed the composition of the loan and deposit accounts rather than been satisfied with the totals.

With the exception of a few pages in chapter 2 of volume 2, the history of the bank is presented in isolation, but to be fully effective the subject demands some comparative analysis. From the point of view of economic performance, it is difficult to draw any conclusions if nothing is known of the rates of return earned by other banks (both foreign and domestic) and by other firms operating as pioneer enterprises in the Middle East and elsewhere. Again, for those interested in management or in business imperialism, it is important to know whether the British Bank of the Middle East was typical of British overseas enterprises or, perhaps, of British enterprise in general. If the attitude and management style of the bank were representative, it is not difficult to understand why the late Victorian period marked the apogee of British economic power and why Britain lost the empire. Despite these criticisms, it is clear that Jones has written a pair of useful, innovative, and informative volumes.

LANCE E. DAVIS

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HENRY MUNSON, JR. *Islam and Revolution in the Middle East*. New Haven: Yale University Press. 1988. Pp. xi, 180. \$18.95.

This book sets out to explain why an Islamic revolution took place in Iran in 1978–79 and why one has not occurred elsewhere in the Middle East or even earlier in Iran. Henry Munson, Jr., begins with a brief survey of Islamic history, concentrating on the way that this history is apprehended by modern Islamic writers. He then examines in detail Middle Eastern countries in which there have been prominent fundamentalist movements in modern times: Iran, Saudi Arabia, Egypt, and Syria. The final section of the book attempts to come to grips with the central question of why the Iranian revolution occurred. Munson examines a number of possible answers, such as rapid modernization, rural-urban migration, economic growth, and frustrated expectations. He shows that each of those possibilities is not an adequate explanation of the revolu-

tion in 1978–79, because each factor existed to an even greater extent in other Middle Eastern countries as well as in Iran itself at other times. Munson's conclusion is that the particular factors that were of importance in Iran were President Jimmy Carter's human rights policy, which undermined the shah's authority, and the Ayatollah Khomeini's charisma and tactical abilities, which united the opposing factions. Unfortunately, these factors are not satisfactory for the same reasons that led Munson to reject the others. Carter's human rights policy affected other Middle East countries but did not produce the same result that it did in Iran; Khomeini was a leading figure in the 1963 disturbances, but those did not succeed in overthrowing the shah.

It is probable that the Iranian revolution was the result of a particular conjunction of a large number of factors. We are, of course, a long way from identifying and understanding all of them. Munson's book is a valuable update on much of the current thinking on the Iranian revolution. It would be a mistake, however, for anyone to regard his conclusions as anything more than provisional.

The author has put together, from secondary works, a wide-ranging survey of religious movements in the contemporary Middle East. His background in anthropology has served him well in highlighting the important area of popular religion, which is too often neglected by those who imagine that what the ulama and Sufi shaykhs teach is the totality of Islam. Munson's use of secondary works is not without its problems, however. They frequently lead him to dubious or inaccurate statements. Just one chapter, for instance, is inaccurate on the following points: the flight of the leading Shi'i ulama to Qum in the early 1920s was only temporary (p. 31); there is no principle that there should be only one *marja' at-taqlid* (p. 32); indeed, for most of the past two hundred years there have been more than one at any one time; the overwhelming majority of Twelver Shi'is have not believed that they should pay the "share of the Imam" to a *mujtahid*; this has been something confined mainly to merchants and traders of the bazaar (pp. 32–33); "strictly speaking," the term "mullah" does not refer only to low-ranking clerics; it would be more correct to say that the term has become downgraded over the course of time (p. 33); and Shi'i ritual does not involve music and dancing (p. 35).

This book is a useful review of an important question for all who are interested in the Middle East. Munson displays a discerning grasp of the issues and a clear and cogent writing style that will be particularly valued by those who do not have a substantial knowledge of the Middle East. The central question of the book is an interesting one and will undoubtedly be the subject of many future works.

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AFRICA

JOHN ILIFFE. *The African Poor: A History*. (African Studies Series, number 58.) New York: Cambridge University Press. 1987. Pp. ix, 387. Cloth \$49.50, paper \$14.95.

John Iliffe has written a very important book. News reports, television documentaries, international conferences, and academic studies all conclude that Africa is today the Continent of Poverty. Most chilling is the fact that its poverty is steadily increasing, with major famines only the most obvious manifestation of a general wrenching decline in people's standards of living everywhere in Africa. Unfortunately, then, Iliffe's book is all too timely.

It is also ambitious. The author believes, unfashionably, that "Africa's splendour lies in its suffering" (p. 1). The activities of kings and their armies and of priests and their shrines are therefore not central to his study. Yet the sources available for writing African history are preponderantly concerned with the actions of members of the elite, people who were decidedly not poor. To overcome this problem, Iliffe has had to be ambitious, compiling in encyclopedic fashion all that he has been able to find out about poverty in the sources he has used. Necessarily, the unevenness of his sources is reflected in an unevenness in the book's coverage and by a certain lack of depth. Areas of Africa that have had relatively rich traditions of research—Ethiopia, Nigeria, the Islamic Sahel, South Africa—are covered in individual chapters. The remainder of Africa is dealt with in chapters that attempt generalizations based on a rather random assemblage of data.

Holding the book together, however, is a basic, overarching hypothesis that is frightening in its implications. Iliffe's thesis is that in the precolonial and early colonial periods poverty was usually caused by random misfortune. One might be stricken with a debilitating disease such as polio or leprosy; a woman might become a widow with few or no children to help her; a person without rights might be made subordinate to another; or one's cattle might succumb to a plague. Poverty was present everywhere, but it was "accidental," and it was also often eased by the charity of those who were not poor and by the fact that land was not in short supply. As Africa became incorporated into the world economy, however, and as colonialism matured, poverty became more and more a structural phenomenon. The impact of capitalism, of labor migrancy, of land alienation, and of the uneven availability of education all transformed poverty from being an "accidental" phenomenon to being the product of one's birth. In a way, poverty was changed from an individual occurrence to a class condition, although a structurally poor person might certainly have his or her poverty compounded by disease or accident. In South Africa this transformation occurred very early; in some parts of Africa, it is occurring only now. But its effects are clear everywhere.

The fact that poverty has become primarily structural means that no longer can charities and philan-

thropy make a substantial difference. Any meaningful action against poverty must be political, and, given the situation in Africa today, there is not much likelihood of such political action being taken. As a consequence, the implications of Iliffe's study are certainly grim. Poverty, which has increased in Africa throughout the twentieth century, will certainly continue to increase, and reports and conferences and studies will continue to label Africa as the Continent of Poverty even as, ironically, various diseases continue to be eradicated.

For the historian of Africa, this book is both stimulating and provocative. By Iliffe's own admission, the history of African poverty is decades away from being written. What is implicit in Iliffe's book is his challenge to other historians of Africa to make the study of poverty, and especially the specific questions of how and when "accidental" poverty became structural poverty, central to their research concerns. One possible topic to which Iliffe has, again unfashionably, directed our attention is the role missionaries and colonial administrators have had in the history of the eradication of accidental poverty from diseases such as leprosy. Another possible area of investigation is the disintegration of the African family over the past several decades, something that, despite a chorus of disclaimers, is a very real phenomenon. Many other topics are brought to prominence in this book as candidates for future research.

All in all, this is an important and, I suspect, a seminal study. It certainly suffers from problems arising from the unevenness of its coverage, a lack of oral testimony, and a certain vagueness about terminology. But rather than cavil at its weaknesses, students should recognize this book as a brave attempt to deal creatively with the history of an urgent world problem and to respond to the intellectual challenges contained within it.

LEREOY VAIL
Harvard University

B. S. HOYLE. *Gillman of Tanganyika, 1882-1946: The Life and Work of a Pioneer Geographer*. Brookfield, Vt.: Gower. 1987. Pp. xvii, 448. \$77.95.

The cover photograph on this protracted book shows Clement Gillman in jodhpurs and pith helmet against the backdrop of a snow-covered crater of Kilimanjaro. Colonial man conquers the African environment. The photograph captures the essence of the book. Brian S. Hoyle, whose interest in Gillman dates back to 1960, has written a meticulous biography of the chief engineer of the Tanganyika Railway and probably the most prolific amateur geographer of interwar East Africa. This is the story of one colonial man, based on the subject's diaries and publications. Yet for all the research and obvious enthusiasm of the author, the biography is ultimately a disappointment.

The historical approach is itself colonial. The author refers to Tanganyikan "tribes" with none of them

"politically advanced" enough to dominate the country (p. 12). The nation is personified: "A modern nation must be aware of its roots . . . if it is to appreciate its true character" (p. 4). Yet Tanganyika hardly intrudes—except its landscape—and its history and peoples are given little attention. In the colonial mode again, we are assured, with little evidence, that Gillman was alert to "African ways of thinking" (p. 216) and exhibited "a very real concern for the welfare of African people" (p. 385).

Who was Clement Gillman and what was he like? Gillman was born in 1882 of Anglo-German parentage, although the prefix "Anglo-" is somewhat misleading, for his education and early life were exclusively continental. He did not visit England until he was twenty-five years old. He arrived in Tanganyika in 1905, shortly after the Maji Maji rebellion, to take up his position as assistant engineer with a German railway company. His incarceration during World War I was followed by a steady rise up the official ladder, this time under British not German rule, until he attained the position of chief engineer of railways in 1928. He retired just before World War II but acted as a consultant to the colonial government until his death in 1946. Gillman was hard to get along with, was convinced of the rightness of his own opinions, seemed impervious to criticism, and, as the author confesses, was "not a particularly original thinker" (p. 396). So why write about him?

Hoyle is primarily interested in Gillman's contribution to the study of the physical geography of East Africa. Gillman undertook extensive research on geomorphology, soil and water conservation, and population and vegetation distribution. He drew on the methodologies of others, particularly those of W. M. Davis (pp. 176-77, 301), and applied them to the East African environment. The problem is that the author gives no clear indication of what Gillman's views were. The contents of his many works are couched in very general terms, and Hoyle is more concerned with how Gillman felt about presenting such-and-such a paper, how the audience responded, and how important the paper was. Geographers still need to consult the original works.

The book, unfortunately, gets bogged down in extraneous detail. Long, convoluted passages from Gillman's diaries are supplemented by descriptions of hotel lodgings, itineraries, the weather, family affairs (including his son's school report on page 201), and even a detailed figure of his World War I prison camp, complete with dormitory layout (p. 123). One footnote informs us (p. 279, n. 20) that "Mrs. Bowis now lives at 26 Stanhope Road, Highgate, from 1931." Much of this should have been excised by a firm editorial pen. Hoyle's formidable research efforts have resulted in a well-organized but nevertheless esoteric volume that is unlikely to appeal to many Africanists, especially at such an extraordinarily high price.

CHRISTOPHER P. YOUE
Memorial University of Newfoundland

DAVID THROUP. *Economic and Social Origins of Mau Mau, 1945–53*. (East African Studies.) Athens: Ohio University Press or James Currey, London. 1988. Pp. xvi, 304. Cloth \$29.95, paper \$15.95.

Since 1985, Mau Mau has again become both a matter of public controversy in Kenya and the subject of several important new studies with more to come. What Mau Mau was exactly—demonic cult, radical nationalist movement, expression of class struggle, a figment of the fevered colonialist imagination, or something else again entirely—remains an open question, as is the issue of its significance for the decolonization and subsequent development of modern Kenya.

When we look at the socioeconomic context from which Mau Mau emerged, we are on firmer ground, however. David Throup's carefully, if narrowly, researched monograph is the most thorough investigation of the official archives and includes the most detailed accounts of several key events that we have thus far. For him, Mau Mau was a response to the policies of the colonial state between 1945 and 1952 regarding the position of Kikuyu squatters on European farms, including the disastrous Olenguruone resettlement scheme; agricultural development and soil conservation in Central Province, particularly in Fort Hall District; and African urban housing and wages in Nairobi. In each area inept and heavy-handed government policies generated resistance from growing numbers of Kikuyu and undermined the tenuous legitimacy of colonial rule.

Throup's account of official policy in Kenya reveals several important factors neglected in earlier accounts. Although Kenya was a notable example of the deliberate social engineering of the post-1945 "second colonial occupation," Throup shows how differently "development" was interpreted in London and Nairobi and in the district *bomas*. Equally important, he documents the influence of the field administration, its conflicts with the specialist departments involved in African development, and its preoccupation with restoring "communal" values and traditional land tenure among the Kikuyu. These factors led to official rejection between 1945 and 1954 of further development of African cash-crop production and an obsession with soil conservation in the Kikuyu reserves.

Lacking in this account, however, are the conceptual and theoretical tools for analyzing the development of these policies within the context of the bureaucratic structures of the colonial state and the state's wider relations with structural forces within Kenya and those linking the colony to the metropole and world economy. The result is a neglect of the elaborate institutional links between the colonial state and settler agriculture—which both shaped officials' notions of development and severely constrained their policy options—as well as the increasingly influential, yet largely unanticipated, growth of industry and international corporate investment.

This lack is reflected, too, in the way in which

Throup deploys his extensive primary sources. All too often a particular sentence or paragraph is followed by a reference listing a long string of archival documents from Nairobi and London relating to the words and actions of numerous officials at different levels of the imperial hierarchy, making it extremely difficult to get any sense of the actual sequences of arguments and decision in the policy-making process. The lack of structural context is also seen in the tendency to focus on personalities, especially Governor Philip Mitchell, and in the provision of an appendix that provides not data on the structural development of the colonial state but biographical sketches of important officials.

The analysis of the cultural and ideological context of policy is also weak, suffering from both the restricted range of sources and the narrow temporal focus of the study; weak, too, is Throup's much sketchier account of the cleavages in Kikuyu society and culture. His study remains firmly in the tradition that has viewed Mau Mau as a rational and instrumental political movement based on specific material grievances. As such, it breaks no new explanatory ground, supplementing but not superseding the analysis of Carl Rosberg and John Nottingham's *Myth of Mau Mau* (1966). The particular form of Mau Mau, its forbidding strangeness to Westerners and its relation to deep divergences of cultural and moral vision within Kikuyu society, remains unexplained. For Throup, who has promised further work on the political sources of Mau Mau, as well as for other scholars, what Mau Mau was remains the unresolved question.

BRUCE J. BERMAN
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SUSAN M. MARTIN. *Palm Oil and Protest: An Economic History of the Ngwa Region, South-Eastern Nigeria, 1800–1980*. (African Studies Series, number 59.) New York: Cambridge University Press. 1988. Pp. xi, 209. \$42.50.

Susan M. Martin's book examines the involvement of the Ngwa, a subgroup of the Igbo people of Nigeria, in the palm oil export trade. It includes a few life histories of burgeoning Ngwa entrepreneurs. The book is based on archival and oral data supplemented with a variety of published and unpublished materials. In analyzing the process of economic change, Martin considers both internal and external factors and challenges dependency and vent-for-surplus theories of economic development, which overplay the role of external influences.

There are eleven chapters in the book, and the author uses a chronological framework in discussing the "profound changes" brought by commerce and colonial rule in the political and social life of the Ngwa people. The early chapters deal with the expansion of trade in palm oil and kernel. According to Martin, the early colonial period "ushered in a new period of prosperity and economic growth for Ngwa farmers" (p. 31), but no convincing evidence is offered.

Yet the period of supposed prosperity was short-lived, as prices of palm produce fluctuated from 1914 on. No longer could Ngwa producers and others manipulate price fluctuations to their advantage. The pragmatic response of the Ngwa to economic adversity was adoption of a new crop, cassava, and Christianity. The link between the adoption of cassava and Christianity and the decline of palm produce prices is curious but adds a new dimension to the argument about the spread of cassava and Christianity among the Igbo.

Perhaps as should be expected, Martin gives particular attention to women and their critical role in the palm oil industry. Although women are presented as the exploited members of society, financially weak and unequal to men, they nevertheless seem to have benefited from the export trade in palm oil and kernel, at least in the early years of colonial rule. As prices of palm produce plummeted, and as colonial policies began to impinge on women's economic life, anticolonial protests became inevitable. Those episodes, the backdrop for the title of the book, are discussed in chapter 9 but unsatisfactorily. Nina Mba's *Nigerian Women Mobilized* (1982) is cited in the bibliography, but her work does not illuminate the discussion.

In the last two chapters of the book, Martin suggests reasons for the collapse of the palm oil industry and the decline of domestic agriculture. She argues that "considerable financial gains" were made from palm produce trade, but "funds were invested in ways that favoured the development of trade and administration, rather than agriculture." Martin thus rejects dependency theories in explaining the roots of Nigeria's current agrarian problem (p. 140).

There are minor issues here and there that specialists might question. For example, the statement that women "retreated back into the household economy and out of the official documentary record" (p. 118) after the Women's Revolt of 1929 suggests that Martin is perhaps unaware of the women's protest movement of 1938, which jolted the eastern Nigerian colonial administration and recalled memories of the Women's Revolt. On the whole, this is a valuable contribution to Igbo studies in particular and to West African economic history in general. It should attract wide readership.

F. K. EKECHI
Kent State University

GEOFFREY JOHNSTON. *Of God and Maxim Guns: Presbyterianism in Nigeria, 1846–1966*. (Editions in the Study of Religion, number 8.) Waterloo, Canada: Wilfrid Laurier University Press. 1988. Pp. 321. \$18.50.

This book is a down-to-earth account of the planning and growth of a Presbyterian church in Nigeria, an institutional history concerned with the deployment and work of the mission and of the evolving African church that grew from it. Much is of primary interest to missiologists—indigenization of theology and ministry,

comity and ecumenism, problems set by institutions such as schools, hospitals, leprosaria, and depots for books and literature. There is material here also for the historian of Africa and of colonialism. Geoffrey Johnston devotes attention to the connection between the mission and the colonial power. Despite the implication in the first half of the title, it looks as if the Scots missionaries were cautious about their governmental bedfellows and the commercial interests behind them.

Johnston has made good use of the local material and his time in the field. (We still do not know how much material was destroyed by the war.) He has followed up his field work with research in the metropolitan archives and with a more recent visit to Africa. He is somewhat overly disciplined as an institutional historian and seldom expands on some of the interesting themes that he raises but that may seem to him sidetracks. For example, he has obviously studied the life and career of Akanu Ibiam, one of Africa's most distinguished church leaders. Indeed, Johnston has interviewed him, but he carefully does not turn his account into an examination of personalities.

In his presidential address to the American Historical Association, titled "Assignment for the '70's" (*AHR*, 74 [1969]: 861–879), John K. Fairbank spoke of the need for more work on the history of missions and of missionaries. Since then much has been done by missiologists and by social scientists. African historians have also done impressive work in this field. With the revival of the *Shi'a dawa'*, the setting up of world mission coordinating centers at Varanasi and Jidda, we may expect more studies. An aspect mentioned by Fairbank that is still neglected by scholars is the effect of missions on the history of a "sending" country. We can look forward to the publication of Johnston's present research, which examines this theme with Canada as the homeland and Shanghai and New Hebrides as the fields.

NOEL Q. KING
Merrill College
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PAUL M. LUBECK. *Islam and Urban Labor in Northern Nigeria: The Making of a Muslim Working Class*. (African Studies Series, number 52.) New York: Cambridge University Press. 1986. Pp. x, 362. \$49.50.

This study about the making of a Muslim working class in northern Nigeria, Paul M. Lubeck admits (p. ix), was greatly influenced by his earlier study of the social history of Europe, which he decided to apply to Islamic Africa. For that purpose he uses an eclectic blend of theoretical tools, combining Marxism and Weberian and world system theories. He employs comparative-historical, qualitative-field, and quantitative-survey methods.

A creative theoretical framework and a rigorous methodology have produced a fascinating analysis of a

multifaceted process of social change in a Muslim society. It proves the vitality of Islam in the transition from tradition to modernity, or that Islam is not merely an ideological residual of a declining precapitalist social formation (p. 287).

A major theme of this study is the articulation of precapitalist institutions with the capitalist labor market. Thus, Qur'anic school networks helped rural to urban migration and continued to operate in a new semi-industrial environment (p. 108). *Mallams*, their sons, and their students became wage laborers but went on to teach and study. They were interwoven as a distinct status group with the first generation of an urban proletariat, one of the very few groups in Africa that had its own intelligentsia (pp. 112, 130).

As intellectuals, *mallams* were respected and looked to for interpretation of social events. They were examples and guardians of Islamic norms of behavior. These *mallams* continued to play their historical role, and the workers in factories, like the common people in Muslim societies in the past, looked to *mallams* for leadership against tyranny and social injustice (p. 151). Hence, a *mallam* who worked in a factory was not only the imam, who led his fellow Muslims in prayer, but also their leader in an industrial dispute (p. 245).

The findings of an empirical survey, expressed in statistical data, convincingly prove that urban Muslims, who had longer wage labor experience, who were literate with longer Qur'anic school attendance, and who were members of Sufi brotherhoods, exhibited greater class consciousness and believed (more than others) that Islam approved industrial strikes (pp. 270–72, 282–86). In other words, Islamic institutions, ideologies, and organizations were strengthened—weakened—by contact with semi-industrial capitalism (p. 277).

Although confined to the context of the city of Kano, and with the specific historical, political, social, and economic conditions of northern Nigeria as background, the analysis and the emerging themes in this work are relevant also to other Muslim societies in Africa and elsewhere. This breadth is what makes this book such an important contribution to the social history of Africa and Islam.

NEHEMIA LEVTZION
Open University of Israel

JANET MACGAFFEY. *Entrepreneurs and Parasites: The Struggle for Indigenous Capitalism in Zaïre*. (African Studies Series, number 57.) New York: Cambridge University Press. 1987. Pp. xiii, 241. \$42.50.

New studies of the large but neglected central African nation of Zaïre are always welcome. Janet MacGaffey's sociological study of African businessmen and businesswomen during the quarter century since the country's independence is particularly so because it challenges accepted ideas on the basis of important original field work. Her detailed interviews in 1979–80 with

African and non-African entrepreneurs in the city of Kisangani (Upper Zaïre province) and in northern Kivu province bring into question the validity of measuring Zaïre's political economy only in terms of the country's state-controlled and corrupt official sector. In discovering "a true capitalist class" (p. 2) operating a second economy outside the direct control of the state and often in opposition to the law, MacGaffey provides a balanced and subtle view of economic and political dynamics in modern Zaïre. As one who spent time in the same places just after MacGaffey's departure, I can appreciate how effectively she captures the spirit of northeastern Zaïre, and I admire her ability to gain access to its commercial netherworld.

Despite the book's fascinating detail, I must note the limitations that are built into MacGaffey's study. Because of the illegality of many activities in a country already rife with suspicion, MacGaffey was able to gather only partial and fragmentary data. Although highly suggestive, her sample of personal narratives is statistically too small to support without question the generalizations that she draws from it. Comparisons with research from elsewhere in Zaïre and from other countries buttress her analysis, but it still wobbles on its legs. Historians, more comfortable than sociologists with fragmentary and anecdotal information, will be frustrated by the fact that MacGaffey must identify her sources by code names (for example, "M. T." and "Madame F."). Future researchers will thus find it difficult to integrate her anonymous subjects into long-term studies of specific family and corporate commercial operations.

Those limitations may be unavoidable, but a second problem with MacGaffey's study lies in her emphasis on class formation as an analytical tool. Although she is to be commended for trying to link events in Zaïre to larger patterns of social change, MacGaffey's small cast of characters seems dwarfed by the size of the stage on which they are presented. Class is most useful as a concept in explaining long-term and large-scale developments, but in this limited study it often seems an ill-defined and unilluminating concept. As MacGaffey admits in the conclusion, other informed students of Zaïre do not agree that distinct classes are forming in the country.

That is not to suggest that the author relies only on class analysis. Indeed, she is particularly effective in highlighting the role of gender in Zaïrian society and in identifying the cohesiveness of certain other social categories. She contrasts, for example, the high degree of cooperation among ethnic Nande businessmen and businesswomen and among foreign businessmen of all nationalities in Kisangani with the lack of solidarity and trust among African businessmen and businesswomen in Kisangani. It would have been useful to probe the particular reasons for such differences more deeply than to trot out "class formation" so often as an explanatory *deus ex machina*.

Even with those significant limitations in the presentation of the thesis, this is still a valuable empirical

contribution to the study of an important and neglected country.

DAVID NORTHRUP
Boston College

JOHN MARKAKIS. *National and Class Conflict in the Horn of Africa*. (African Studies Series, number 55.) New York: Cambridge University Press. 1987. Pp. xvii, 314. \$44.50.

John Markakis is well known to students of the Horn of Africa as the author of a seminal book on the social anatomy of traditional Ethiopia and the coauthor of a searingly critical study of the cataclysmic upheaval that swept Ethiopia in 1974. The upheaval brought to an ignominious end a two-thousand-year-old imperial rule and ushered in a brutal leftist military regime to govern that unhappy country. With this book Markakis has undertaken a more ambitious task than he attempted in his earlier work: nothing less than a complete regional study of the entire Horn, focusing, as the title implies, on the violent clashes of class and nation that have engulfed Ethiopia, Somalia, and the Sudan for more than two decades, turning these countries into the "Lebanons" of the Horn—a strife-torn region where perennial genocidal wars, drought, famine, and the attendants of death and starvation have produced the apocalypse. If Markakis's analysis is to be believed—and painfully he leaves no room for doubt—society in the Horn is sliding toward collective demise.

The great merits of the book, to my mind, rest on two exceptional contributions. First, the comparative approach, as an analytical tool, works rather well for the author, enabling him to give us, at a glance, the common elements as well as the differences in the various epic battles: the Anya-nya movement of the southern Sudan versus the Arab north, the various dissident movements versus the central Ethiopian government, as well as the Somali National Movement and the Somali Salvation Democratic Front versus the Somali regime. Commenting on the tendency of modern scholarship to be monographic and narrowly focused, Gore Vidal, with his usual ascerbic wit, has sneeringly dismissed contemporary academics as "scholar-squirrels." In the case of Markakis's book, Vidal's charge is difficult to sustain.

The second great merit of the book lies in its wealth of detail. This feature is the more gratifying in coverage of a region where illiteracy predominates and documentation remains woefully scarce. Through numerous interviews with local informants and "on-site" field inquiries, Markakis accumulated a handy cache of material and has deployed it well to reconstruct an excellently documented story. As an Ogadeni myself, I was delighted to discover, for example, that Markakis's net managed to catch such obscure organizations as the Ogaden Company for Trade and Commerce and the Nassir Allah (p. 175), two shadowy, anticolonial move-

ments of the Ogaden Somalis in the 1950s in whose dubious activities I was personally involved.

In view of Markakis's manifest achievement, it may seem churlish to complain. Yet I must confess to, in my view, an important complaint. Markakis's study, as he readily admits early in the book (pp. xvi–xvii), is informed by a materialist (Marxist?) mode of analysis. This leads him to concentrate throughout on "the material dimension" of issues and events. As a result, material categories—class contradictions, production relationships, and the role of the state—are overemphasized to the detriment of the idiosyncratic and unpredictable in human behavior. In fact, the words "class" and "national contradictions," both darlings of leftist writers, jump out at the reader from almost every page. One unfortunate consequence of this emphasis on "structures" at the expense of "persons" is that we learn precious little from this book about the troika of dictators who have dominated life in the Horn for so long—General Muhammad Siyaad Barre of Somalia, Colonel Mengistu Haile Mariam of Ethiopia, and the now-fallen Colonel Jaafer al-Nimeiry of the Sudan. In his otherwise embarrassingly obsequious portrayal of Mengistu (*Ethiopia: An Heretical Revolution* [pp. 275–83]), the journalist Rene Lefort provides a credible theory of the little colonel's secret of success. It is a pity that a methodological predilection seems to have distracted Markakis from giving us the benefit of his informed analysis into the methods and means by which the three dictators seized power and triumphantly retained it—against all expectation. But this minor critique should not detract from the achievement of a work that is clearly "must reading" both for students of the Horn and for policy makers.

SAID S. SAMATAR
Rutgers University

MICHAEL CROWDER. *The Flogging of Phineas McIntosh: A Tale of Colonial Folly and Injustice, Bechuanaland, 1933*. New Haven: Yale University Press. 1988. Pp. xii, 248. \$27.50.

In September 1933 a bizarre incident occurred in a remote backwater of the British empire. An impoverished, young, white wagon builder named Phineas McIntosh, accused of persistent unruly, drunken, and debauched behavior, was sentenced and flogged for his misdemeanours. The place was the Bechuanaland Protectorate (now Botswana), South Africa's western neighbor; the judge was Tshekedi Khama, regent of the Bangwato people. He was black.

It was not a custom in the British empire for blacks to flog whites, least of all in southern Africa, where color consciousness was always acute. The imperial response was swift and resembled something out of Gilbert and Sullivan. Two hundred fully armed British marines were dispatched a thousand miles by rail from Cape Town to the Kalahari Desert to uphold the white man's prestige. A kangaroo court was summoned, during

which three howitzer guns were constantly trained on Tshekedi. Denied legal counsel, he was deposed and banished to the far north. From there he orchestrated such an effective campaign that within three weeks he was back in office. The British officials responsible for the grotesquely inappropriate show of force toward a totally unrebelling people were made to look exceedingly foolish.

Michael Crowder recounted this extraordinary story with characteristic elegance and wit. Crowder was incapable of writing a dull book. Here he brilliantly illuminated a particular colonial experience in a way that both invites comparison with colonial situations elsewhere and remains easily accessible to the nonspecialist.

It is striking to observe how much power African chiefs in Bechuanaland possessed in contrast to chiefs in neighboring South Africa and Southern Rhodesia. Race relations, too, were far more relaxed than elsewhere in the region. There was much interracial sexual activity, and the entire white population of Tshekedi's territory, including McIntosh's parents, "filed past Tshekedi and shook his hand and told him how sorry they were for what had happened and hoped he would soon return as Chief" (p. 93).

Even white South Africans, whose racial sensitivities were cited as the main excuse for sending in the marines, tended to side with Tshekedi against McIntosh and the administration. Tshekedi shines through this remarkable book as an example of how effectively an educated chief, with good connections in the legal and missionary worlds—he had visited England in 1930 to present a case to the Privy Council—could manipulate the space available to him. As Crowder wrote, "Tshekedi continued to call the tune. Indeed, throughout the whole McIntosh saga and its aftermath it was around Tshekedi that events revolved, and for much of the time it was he who determined their direction" (p. 195). That contrasts starkly with what was possible in South Africa and Southern Rhodesia. Tshekedi's successors in contemporary Botswana, aided by new-found mineral wealth, continue to exploit the space available to them in the political crisis of southern Africa and to act as an alternative, democratic model to their powerful and predatory neighbor.

Shortly after this review was written, Michael Crowder died in London on August 14, 1988, aged 54. He was a loyal and caring friend to many, in good times and in bad. He will be deeply missed.

ROBIN PALMER
Oxfam, Oxford

ROBERT B. EDGERTON. *Like Lions They Fought: The Zulu War and the Last Black Empire in South Africa*. New York: Free Press of Macmillan. 1988. Pp. xii, 244. \$22.95.

Isandlwana! Rorke's Drift! Ulundi! Those are names that have inspired historians and novelists for over one hundred years to tell the story of Britain's greatest

imperial misadventure against a reluctant Zulu foe. In 1965 Donald Morris wrote a definitive and highly praised account of this Zulu war, which he titled *The Washing of the Spears*. That very readable book delayed the completion of this version by over two decades. In the interval, however, many excellent primary sources have come to light, such as letters, diaries, and testimonies of British soldiers and interviews with Zulu soldiers. It is on that new evidence that Robert B. Edgerton builds his insights into Britain's most resounding defeat by an African king and its persistence onward to vindicate itself by defeating the legendary Zulu empire. But this work is more than simply an analysis of fresh sources. Edgerton draws on his anthropological and psychological training to supplement conventional explanations of military tactics and strategy with an interpretation of the motives and behavior of the combatants, both officers and fighting men, on both sides of the conflict.

In seven chapters of elegant prose, Edgerton relates the story of a tragically flawed war from the first British defeat at Isandlwana, caused by British contempt for African fighting ability, through the dogged pursuit of victory over an enemy who was trying desperately to sue for peace. The reader will find little to fault in these interesting and insightful chapters.

The author then turns to an analysis of the nature of this war for each of the combatants. He analyzes the British style of warfare through the eyes of both the officers and enlisted men, each of whom had different motives, training, and experience. He then does the same for the Zulu. He explains the role of warfare in the social structure of each of those societies and provides an interesting explanation of how both British and Zulu armies fought with such ferocity. He proposes that both armies were trapped by their conventions into using tactics that served them poorly: the British used the square formation, the Zulu the ox-horn formation.

Edgerton concludes his book with a chapter describing colonial wars throughout Africa. Although it is a useful reminder of the violent nature of French, German, Belgian, and British imperialism and of the fact that Africans resisted conquest at a very high price, the chapter comes off as a digression that is rather sketchy and inadequately integrated into the thesis of the book. It almost appears to be some interesting preliminary research the author composed and did not want to waste.

The major discordant notes in this book are the author's inconsistent use of modern and archaic orthography for African terms and names and his inconsistent pluralization of African names. One might also challenge his brief introduction to the rise of the Zulu state, but those are minor problems.

This book, the title of which refers to the Zulu description of the British mode of fighting, is a continually fascinating and useful account of the motivations and behavior of both the African and the

British fighters in this important imperial war in South Africa.

WILLIAM F. LYE
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GAVIN LEWIS. *Between the Wire and the Wall: A History of South African "Coloured" Politics*. New York: St. Martin's. 1987. Pp. 339. \$45.00.

As Gavin Lewis concedes toward the end of this book, the life of Abdullah Abdurahman, leader of the African People's Organisation (APO) from 1905 to 1940, "looms so large in the history of organised Coloured politics . . . that for the historian he threatens to blot out all other features of the political landscape" (p. 198). Indeed, Lewis's study is very largely a narrative of the APO's development under Abdurahman's leadership. Seven of the nine chapters detail the organization's history from its emergence in the 1900s, spurred by the threat contained by union to existing "coloured" rights, to its decline into factionalism in the 1940s. During that period the APO's strategy of mobilizing coloured voter support behind sympathetic white parliamentary candidates marginally influenced the outcome of elections, and, in consequence, the APO retained political leverage and hence popular credibility. As late as 1938, deputations, torch-lit parades, and days of prayer were employed successfully to resist the efforts of the Cape Provincial Administration to impose residential segregation in the Cape peninsula.

By the beginning of World War II though, the APO's calculated mixture of rhetorical stridence and tactical caution was losing effect. Extensions of white suffrage in the 1930s, notably the enfranchisement of white women, reduced the significance of the coloured vote. The APO's inability to check the impact of white labor preference policies and the diminishing returns of white liberal clientalism strengthened more radical voices within the coloured political elite. The National Liberation League (NLL) was founded in 1935. Influenced by Marxists, the league rejected the APO's understanding of segregation as an import from the reactionary northern provinces imposed on the Cape's liberal heritage. Instead, the league called for working-class unity in the face of racial capitalism. One faction of the league, however, led by Abdurahman's daughter Cissie Gool, maintained a strategic continuity with the APO. Under Gool's leadership, the NLL attempted to switch coloured voter support from liberals to Communists, a new set of white patrons who had begun to contest elections during the war.

Gool herself succeeded her father as Cape Town's only black municipal councilor and within the council demonstrated her essential commitment to pragmatic reformism. Abdurahman's death in 1940 robbed the APO of strong leadership, and the APO and its satellite, the Teacher's League, became the sites of fierce conflict between Communist-oriented moderates led by Gool and the purist advocates of noncollaboration and

principled unity. The purists, represented successively in the New Era Fellowship, the Anti-Coloured Affairs Department Committee, and the Non-European Unity Movement, predominated in coloured politics in the early period of apartheid in the 1950s. Their achievement included the elevation of the boycott to a strategic principle and the introduction of an unprecedented ideological fratricide that negated any effort to organize resistance to government policies within the coloured community until the 1970s.

This is the first full-length academic study of South African coloured political history. It includes a sensitive appraisal of the first generation of coloured political leadership. One of the most strongly developed themes concerns the problematic social identity of the coloured community and the extent to which coloured politics buttressed or resisted an externally imposed identity. The internecine quarrels (and they were often literally that) between the different factions during the 1940s are traced with admirable clarity and considerable sympathy. The narrative speeds up after the 1930s, and the developments since 1948 are sketched very schematically indeed. The emphasis is therefore on the APO rather than on the organizations that later eclipsed it. This focus is a pity, for although the Unity movement was unable to mobilize a mass following (and the reasons for this are most convincingly diagnosed), its intellectual life was substantial and interesting. Why coloured teachers, incidentally, were able and motivated to create such a vital alternative subculture as was expressed in the Unity movement is an issue that deserves more attention than it receives here. Similarly, the publication for twenty years of the Unity-influenced *Torch*, probably the most entertaining polemical newspaper to have appeared in South Africa, rates more space than the bare mention of its first appearance in 1946. Finally, even during the APO's ascendancy, more radical organizations could exert a powerful counterattraction. Although Lewis acknowledges this fact with a succinct but well-researched chronology of efforts between 1927 and 1931 by Communists and the African National Congress to build a militant following among western Cape farm workers, his coverage of working-class as opposed to elite coloured politics is somewhat patchy. In particular, James La Guma, who flits elusively in and out of Lewis's narrative, deserves a more commanding position and detailed biographical discussion.

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HELEN BRADFORD. *A Taste of Freedom: The ICU in Rural South Africa, 1924-1930*. New Haven: Yale University Press. 1987. Pp. xvi, 364. \$40.00.

Like many other historians writing on popular movements in South Africa, Helen Bradford seeks to reconstruct the history of black resistance in part to illuminate current political issues. Unlike many earlier

writers, however, she has not produced a study of leaders and institutions in the more accessible urban centers; instead, she analyzes the continual reinterpretation and reshaping of the Industrial and Commercial Workers' Union of Africa (ICU) in response to particular local circumstances in the countryside, where the union ignited a dramatic wave of radical protest during the late 1920s. In this concern with understanding rural social and economic relationships, Bradford joins some of the most able and imaginative social historians of South Africa of recent years.

Bradford briefly traces the history of the organization to its launching in 1919 by Clements Kadalie, a Nyasaland-born activist who greatly admired Marcus Garvey. But Bradford concentrates mainly on the last part of the decade, when the union swept across vast stretches of rural South Africa, attracting at least one hundred fifty thousand members and transforming its orientation from narrow trade-union concerns to wide-ranging agrarian and nationalist issues. The main chapters trace the nature of social and economic relations in the countryside during the 1920s; the leadership, organization, and ideology of the movement; and the ICU's lively, often stormy, history in three diverse local areas: the Transvaal, the Natal Midlands, and the Transkei.

In constructing her underlying argument, Bradford carefully and thoughtfully evaluates the strengths and weaknesses of the movement and its leadership, taking care not to gloss over the movement's extremely complex, contradictory nature. The ICU developed in the 1920s, a time when racial oppression in South Africa was intensifying and when the increasing capitalization of agriculture threatened established relationships between white landowners and rural blacks. Under these conditions, the union attracted young, educated men from middle-class Christian families into positions of leadership, men whose prospects for the future appeared insecure, and the union drew its membership, holders of the famed red tickets, mainly from male labor tenants in imminent danger of proletarianization. These "people in the middle" of the black social hierarchy in the countryside were likely to be uneducated but distanced in many respects from more "traditional" members of their communities. They often belonged to independent Christian churches whose doctrines and organization expressed a critical stance toward a white-dominated world. For other rural inhabitants such as sharecroppers and squatters, protest was too risky; for tightly controlled migrant laborers, too difficult. Yet labor tenants were vulnerable, too, and Bradford concludes that, above all else, it was evictions from farms that eventually put an end to ICU-inspired protest.

But the movement also contributed to its own demise by its extravagant promises, sometimes generating millennial expectations of black American liberators, and by its failure to channel popular enthusiasm into lasting programs and structures. Instead, leaders relied on massive meetings addressed by stirring and flamboyant speakers to bind members to the group, seeing them-

selves more as "deliverers of the people" than as revolutionaries committed to empowering the rural poor. Yet, for a brief moment, the ICU succeeded, on a scale no subsequent organization has matched, in mobilizing rural South Africans into a powerful movement that combined their aspirations for land and national liberation.

Meticulously documented in both oral and archival sources, written in a lively and engaging style, and balanced in its assessment of the ICU's achievements and weaknesses, Bradford's book restores to rural South Africans a critical chapter in their history. Without this analysis, only the shell of the union can be understood. Bradford takes care to analyze the more elusive symbolism of the ICU's millennial offshoots as fully and sympathetically as she does its political and economic content, which is more accessible to outsiders. Clearly an important contribution to African and South African historiography, this book also should interest other historians concerned with rural social relationships and political movements.

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DENIS TWITCHETT AND MICHAEL LOEWE, EDITORS. *The Cambridge History of China*. Volume 1, *The Ch'in and Han Empires, 221 B.C.-A.D. 220*. New York: Cambridge University Press. 1986. Pp. xli, 981. \$110.00.

Although this volume is the seventh to appear of a projected fifteen in *The Cambridge History of China*, it is in fact the initial volume in the series. At first sight the title, specifying as it does China's earliest imperial regimes, is so unexpected as to appear discrepant. A reader's immediate reaction must be to ask what has happened to that millennium or more of the recorded Chinese past that laid the foundations not only of Ch'in and Han society but also of their institutions, ideas, beliefs, and artistic culture *au sens large*. The editors' response is that the lack of a generally accepted reconciliation of recent archaeological evidence relating to that early period with the traditional written record makes it impracticable at this time to prepare a historical synthesis of lasting value. I can only leave the reader to decide whether or not that argument justifies relegation of the formative third of Chinese history to discussion "elsewhere, and at another time" (p. v).

On the general pattern of all Cambridge histories, this book consists of sixteen chapters contributed by specialists under the joint guidance of the volume's two editors, Denis Twitchett and Michael Loewe. The stated aim of the series is to offer the general reader "a summary of the information that is available in the primary sources in the light of the most recent critical scholarship" (pp. xvii, 2). To that end, in the present volume eleven authors have been laid under tribute, six of whom may be said to have grown old (and, in one

instance, died) in the service of Han. Together those six are responsible for a total of eleven chapters. To the extent that the authors are distilling the reflections of a lifetime, it is not to be expected that their contributions will break entirely new ground or present significantly new points of view. Nor do they, although in one or two instances the authors modify earlier views.

Despite the circumstance that nine chapters are devoted to political history and the history of political institutions, social, economic, and intellectual history as well as law, philosophy, and religion are treated fairly fully in the remaining seven chapters. Art, literature, economics, and technology are not considered except for incidental remarks *en passant*, and, despite both an editor and a contributor who began their academic careers as geographers, the physical environment of the Chinese cultural realm is ignored (in marked contrast, be it noted, to *The Cambridge History of Iran*). As in the other published volumes, the contributors draw eclectically on Chinese, Japanese, and European scholarship but not, in this case, on Russian. The section on Nan-Yüeh in chapter 6 is an instance where Russian secondary literature would have proved helpful, for Victor Velgus's interpretation of the expedition to Huang Chih is a signal advance on my exposition in *The Golden Khersonese* (1961).

The Ch'in state is dispatched in a single chapter by Derk Bodde, whose first important work on the topic was published exactly half a century ago. His compendious summary of presently available information is thoroughly representative of the best historical writing by the first generation of American sinologists. Thereafter, apart from some discussion of Ch'in law in a subsequent chapter, the volume is wholly concerned with the Han dynasties, and the Wang Mang interregnum. In chapters 2 through 6, Loewe and Hans Bielenstein furnish a conspectus of Han political history, Mansvelt Beck offers a conventional account of the fall of the dynasty, and Yü Ying-shih provides a survey of its foreign relations. In chapters 7 and 8, Loewe and Bielenstein discuss the administrative structure of the two Han dynasties, and, in chapter 9, A. F. P. Hulswé presents a straightforwardly factual account of Ch'in and Han law as an arm of government. In chapters 10 and 11, Nishijima Sadao and Patricia Ebrey provide excellent discussions of the economic and social history of the Former and Later Han, although Sadao's treatment of agriculture—because it was completed as long ago as 1969—has been rendered partially inadequate by the publication of Francesca Bray's *Agriculture* (1984), part 2 of *Science and Civilization in China*, volume 6.

The final five chapters are devoted to intellectual and religious history, although it is perhaps symptomatic of the way the volume is organized that Loewe's discussion of the concept of sovereignty occurs here rather than among the earlier chapters dealing with governmental institutions. Loewe's final contribution sets in place a descriptive framework for the succeeding rather uninspired chapters on the development of the

Confucian schools (by Robert Kramers) and philosophy more generally during the Later Han (by Ch'en Ch'i-yün). The concluding chapter is a masterly study by Paul Demiéville of philosophical and religious thought from the Later Han through the northern and southern dynasties to the Sui. Because it was written in the early 1970s, it has been deemed necessary to update this chapter by means of a postscript.

The individual chapters of this book provide the general reader, for whom they are written, with a great deal of sound information, even though they vary considerably in quality. But apart from Ebrey, the authors display little awareness of contemporary trends in political and social analysis. Moreover, there is a looseness, almost an ad hoc quality, in the structure of the volume that is manifested most noticeably in the overlaps between chapters, perhaps even in the order of the chapters, in the updating of essays written nearly twenty years ago by means of a postscript and editorial additions, and in the fact that one of the essays runs on for some four centuries beyond the fall of Han. It is as if the editors availed themselves of materials at hand rather than insisting on tightly integrated contributions. The editors, however, have provided the reader with valuable aids to understanding in the form of a schedule of official titles and institutional terms, a table of Han weights and measures, an annotated list of Han emperors, and a glossarized index. It should be noted, though, that the bibliography is restricted to works cited in the footnotes to the text.

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DIETER KUHN. *Die Song-Dynastie (960 bis 1279): Eine neue Gesellschaft im Spiegel ihrer Kultur*. Weinheim, FRG: Acta Humaniora, VCH. 1987. Pp. xvi, 528. DM 162.

Volumes devoted to a single dynasty and yet addressed to the general reader are relatively rare in Chinese history; so, too, are cultural histories. For these reasons as well as for the intrinsic appeal of the dynamic Song (older transcription: Sung) dynasty, 960–1279, this is a welcome volume. In the view of one school of historians, the Song dynasty marks the emergence of China's early modern era, characterized by a new bureaucratic elite, accelerated economic growth, a new level of urbanism, significant technological developments (especially printing), and movement into new intellectual and artistic directions. (The word "renaissance" is occasionally applied to the era of the Song dynasty but does not really work because there was nothing from which China was then reawakening.) That era is also the first period from which voluminous sources can be said to have survived. Dieter Kuhn's book provides ample evidence for all of this. Kuhn, a historian of Chinese technology and in particular of textile technology, draws heavily on the recent research of Chinese, Western, and Japanese scholars to produce a book that probably would have been impossible to write just

twenty-five years ago. It is also a beautifully produced volume, containing 171 illustrations of striking variety and 19 maps. Ample bibliographical references (though only in notes, not listed in a separate section) and a Chinese glossary are provided.

Cultural history, perhaps even more strikingly than other varieties of history writing, defies any easy, clear-cut definition. Obviously, the task of treating in a single volume an advanced and rather well documented culture imposes severe limitations of choice on the historian. Kuhn's solution is to concentrate on institutions and material culture and generally (though not uniformly) leave aside the more abstract aspects of Song civilization. In successive chapters, therefore, he takes up political structure and the nature of the elite, agriculture, land tenure, urban life and culture, and various aspects of material culture. On the whole these discussions represent clear and up-to-date syntheses. The least satisfactory is the chapter on land tenure, allotted only twenty pages and limited to the Northern Song period, which ended in 1126. Admittedly, this topic includes the thorniest of issues. Whereas the solid treatments provided of the state, of the elite, and of urban developments have their counterparts in other Western-language publications, those on agriculture and material culture introduce much new and valuable information. The author demonstrates not only the wide range of products and goods produced on the land and by craftsmen but also the pertinent technologies employed. As expected, his discussion of the silk industry is especially strong; he treats the whole process from worm to fabric in fascinating detail.

Given his self-imposed constraints, Kuhn does not find it necessary to take up such major areas as Song literature, thought, and religion. Needless to say, those are significant omissions. Oddly enough, however, he even fails to consider the literary, intellectual, and spiritual matters that touch directly on his primary concerns. For example, no mention is made of the "lyric" poem (*ciltzu*), which emerged during the Song dynasty as a major literary form out of the gay quarters of the cities and towns. Although theater is briefly mentioned (pp. 269–70), the first examples of vernacular drama and fiction are also ignored. Astonishingly, no mention is made of neo-Confucianism, the major intellectual movement of the period, though the ideas it stimulated determined the character of the civil service examinations (pp. 112–16) and the agenda for the reform effort (pp. 99, 204–09). The descriptions of archaeological finds (pp. 18–56, 392–430) are wonderfully rich and revealing but, similarly, are not accompanied by a treatment of religious practices and beliefs. In short, Kuhn's perspective on human culture is often unnecessarily narrow and concrete. He also fails to consider developments in sufficiently broad diachronic terms; with due acknowledgement of limitations of space, the Tang background (618–906) of many of the Song achievements is largely neglected. (Symptomatic is the quite unsatisfactory introductory chapter, diffuse

in character and focused exclusively on the tenth century.)

Yet, despite these deficiencies, the strengths of the volume are many, and much of the material presented is invaluable. Kuhn offers us here one of the best introductions to Song China and certainly the best to its material culture.

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RICHARD VON GLAHN. *The Country of Streams and Grottoes: Expansion, Settlement, and the Civilizing of the Sichuan Frontier in Song Times*. (Harvard East Asian Monographs, number 123.) Cambridge: Harvard University Press. 1987. Pp. xxii, 305. \$24.00.

This book is an in-depth study of a frontier prefecture, Luzhou in southwestern China, during the Song dynasty (960–1279). Richard Von Glahn goes into great detail, using the extant source materials, such as local gazetteers, to describe Song expansion and settlement in the Luzhou area, and occasionally relating the specific development of Luzhou to the larger picture of Song social, political, and economic developments.

In early Song times, society in the Luzhou area was dominated by minority chieftains and local Chinese magnates whose power was based on the personal bondage and loyalty of their clients. Salt production was a major motive for Song expansion into southern Lu. A cycle of expansion and settlement from 1070 to 1120, when the new party adopted an aggressive frontier policy, brought about centralized bureaucratic organization and control, which diminished the power of the local magnates and facilitated the transformation of minority groups into farmers.

Luzhou's society evolved from what the author calls a "border" to a "periphery" and eventually to a "hinterland" in the twelfth century (pp. 216–20). That evolution was made possible by Sichuan's demographic growth, which increased metropolitan (Chengdu) demand for food, fuel, timber, and other commodities that Luzhou produced. The changes marked a reorientation of Luzhou economy from subsistence to commercial production and effected a transition of most parts of the prefecture from a pioneer society to the dominant Han Chinese social order. In conclusion, Song frontier expansion and settlement significantly altered the frontier, resulting in the establishment of Chinese sovereignty, the disruption of native society and polity, and the incorporation of the frontier into the Sichuan regional system. Moreover, based on his empirical study, the author advances a critique of the models of regional systems proposed by G. William Skinner and Robert M. Hartwell (Skinner, *The City in Late Imperial China* [1977]; Hartwell, "Demographic, Political, and Social Transformations of China, 750–1550," *Harvard Journal of Asiatic Studies*, 42 [1982]), although it seems that more case studies are needed to reconsider their conceptual approaches.

This work is uniquely important because it is a pioneer attempt to study a southwestern prefecture rather than a region in the north or northwest where the foreign menace to China was greatest. The descriptions of the life styles and customs of the minority groups are valuable. Flaws are few, one of which is a mistake on the map on the back endpaper: Lingying should be Yingling. Since the author does not compare the aggressive Song policy with policies elsewhere, for example, policy toward the Hsi Hsia, his details, including too many places and personal names, of Song military campaigns against minority tribes are not of interest to the reader.

JING-SHEN TAO
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J. Y. WONG. *The Origins of an Heroic Image: Sun Yatsen in London, 1896-1897*. (East Asian Historical Monographs.) New York: Oxford University Press. 1986. Pp. xviii, 330. \$26.00.

Revolutions are inspired in part by the image of a heroic leader. Whether it is the Russian, Chinese, Cuban, or Vietnamese revolution, Vladimir Lenin, Mao Zedong, Fidel Castro, and Ho Chi Minh loom almost as large as the process itself. Historians differ over how important any individual is to a revolution, as they do over how a heroic image is created. Does the leader purposefully create the image or do circumstances or other people do it for him? Such questions are critical to our understanding of the interplay of personalities, process, and mass participation in a revolution. J. Y. Wong investigates that historical confluence, emphasizing the emergence of Sun Yatsen's heroic image. He argues that the image arose during Sun's kidnapping in London and its immediate aftermath. Wong challenges some conventional wisdom, settles some disagreements among scholars, and reaffirms some old conclusions. He also opens some new areas of investigation.

Just before Sun died on March 12, 1925, he whispered, "peace, struggle . . . save China." Although his dream for China never fully developed, his image as a hero did, and it contributed significantly to China's unfolding revolution. That image was created years before Sun's death.

Wong concludes that the image was born during 1896-97. Through the development of an integrated methodology that builds on previous analysis, the exploration of written and oral sources, and the use of field research, the author describes a revolutionary who was dedicated to China's transformation and was at the same time pushed along by events, friends, and comrades. Sun's heroic image was designed primarily by others, including James Cantile and Chen Shaobai, who purposefully promoted the image, and by indirect forces, including the British press and the fearful and inept Chinese minister Gong Zhaoyuan. Wong's case is

most revealing and provocative when he concludes that Cantile, not Sun, is the author of *Kidnapped in London*.

Sun emerges as neither manipulative nor impervious to his emerging status. He believed it was his duty to help "his people downtrodden by the Manchus. . . . Having, it seems, worked out in his mind the Three Principles of the People, he was all the more confident about his ability to lead" (p. 289). If nothing else, the kidnapping incident provided other people with the opportunity to promote Sun's image and to stimulate Sun's faith in himself and in China's destiny. Those conclusions provide the basis for subsequent historical judgments about China's revolution, and for now they resolve some of the more pressing questions regarding Sun's role in modern Chinese history.

Wong also tackles other questions regarding Sun's experiences in London. Did Sun, for instance, stroll into the Chinese embassy, or was he abducted? Wong warns, "I may be wrong, since all the evidence that I have assembled so far has not enabled me to reach absolute certainty on that point" (p. 294). He concludes that Sun was kidnapped and that he did not distort or twist what happened. Although Wong is correct that the definitive answers to the kidnapping and other questions must await further evidence, he has significantly advanced our march to those answers.

Wong is detailed in his research and his narrative. Although there are bound to be disagreements with minor points of Wong's analysis, his volume is a major contribution to our understanding of Sun Yatsen as a revolutionary and to our knowledge of China's slow transformation.

EDWIN CLAUSEN
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PAUL CLARK. *Chinese Cinema: Culture and Politics since 1949*. (Cambridge Studies in Film.) New York: Cambridge University Press. 1987. Pp. vii, 243.

Literature has long been a well-traveled path for those explaining the cultural history of the People's Republic of China in its social and political context. Only with the much freer access of the post-Mao era, however, have Western scholars been able to do this in the visual and performing arts.

Film history is a notable addition to this broadening view of China's recent past. Formerly, few Chinese films were available to foreign reviewers, and, thanks to shifts in the political line, many of the more interesting ones were not long available to Chinese audiences. Only Jay Leyda, who worked in the Beijing film archives in the early 1960s, had the necessary access to write a general history of Chinese cinema (*Dianying: Electric Shadows; An Account of Film and Film Audiences in China* [1972]). His book, although cinematically astute, was rather limited in its understanding of Chinese culture and politics. Paul Clark's study, therefore, breaks new ground, even if the essays in the collection edited by Chris Berry, *Perspectives on Chinese Cinema*

(1985), which includes a significant piece by Clark himself, prepared some of the way.

The present book is more detailed and more comprehensive, starting with a chapter on film and society before 1949 and concluding with China's "new wave" films of the early 1980s. Throughout his generally chronological narrative, Clark relentlessly pursues three themes. The first is the Communist party's conscious use of film to create a standardized mass national culture. More interesting and more intricate is the three-way relationship between party authorities, film makers, and the viewing public. And, finally, Clark examines the tension between the politicized role of art as defined by Mao in Yan'an before 1949 and the more sophisticated views of the modern intellectuals who grew up in the cosmopolitan environment of prerevolutionary Shanghai.

It is hard to find fault with this choice of themes, and Clark makes particularly good use of the Yan'an-Shanghai dichotomy in sorting out differences within and between the party's cultural apparatus and the film-making community. This approach is useful for destroying previous assumptions about a clear-cut and unmitigated tension between politics (the party) and culture (the creative intellectuals), although the revelations from China after the Cultural Revolution made such a simplistic view untenable.

Similarly, Clark's attention to "Chinese qualities" based on shared cultural perceptions by commissars, intellectuals, and audiences deepens our understanding of both cinematic and general cultural history. For example, the stock types in most films are partly a socialist-realist search for typicality and partly a continuation of conventions derived from the traditional Chinese stage and popular literature. Similarly, the precedence of film writer over director in shaping the final work is a function of both political censorship and traditional respect for the written text.

Yet, for all of these solid virtues, the book is not as entertaining as one might expect. Perhaps this is because Clark's discussion of specific films is often rather cursory in order to fit the entire history into 184 pages. But there is also something prosaic about Clark's style that reminds one of the slow pace and static camera work that he notes in most Chinese Communist films.

The research is there; the material is good; the conclusions are sound. But, when reading the book one could wish for faster cuts, some more novel camera angles, and a less mechanical repetition of basic themes. On the liveliest of the arts, solid scholarship need not be so wooden.

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EMIKO OHNUKI-TIERNEY. *The Monkey as Mirror: Symbolic Transformations in Japanese History and Ritual*. Princeton: Princeton University Press. 1987. Pp. xiv, 269. \$29.95.

On the dust jacket of this intriguing book, anthropologist James Fernandez claims that it "makes an important contribution to theory in history and anthropology as well as to Japanese studies." The author, Emiko Ohnuki-Tierney, is also an anthropologist. She states that the book will examine "structural transformations but also historical changes" (p. 11) in the image of monkeys in Japan, demonstrating shifts in the Japanese appreciation of self and other as well as of special status people (*burakumin*) within Japan. This is a heavy order for a book about monkeys.

The author adopts an interesting strategy: she uses the changing metaphor of monkeys to illustrate modifications in Japanese history and culture. She has done extensive field work on monkey performances in Japan. That is a promising line of inquiry into how some Japanese look at the world and at society. It is also promising for Japanese studies that Ohnuki-Tierney stresses the importance of history for anthropology, rejecting the tendency to rely on the "ethnographic present" (p. 3) to the detriment of understanding culture as a diachronic process.

Indeed, the case study that makes up the bulk of this book offers an engaging approach to Japanese culture viewed across several historical periods. The monkey figures as a reflection of human traits in Japan as elsewhere. Its changing persona is "a lot of fun" to watch, as the author notes (p. xi). We get to see the monkey as a metaphor for humans (both as clown/trickster and as scapegoat) and as a basis for performance art extending down to the present day.

But there are problems on the theoretical front that call into question the value of this book for the study of history. We are told that a chronology will be used that some specialists "will undoubtedly . . . find detractive" (p. 12). Periodization has always been a matter for debate, but normally historians do not reify it into a leading historical actor in its own right, which it becomes in this book. Other assertions in the chapter on theory also raise problems. Successive paragraphs sometimes appear contradictory, which happens when the author discusses the scope of the book and "its de-emphasizing intra-cultural variations" only to follow immediately with the statement that "a major focus of this book is indeed intra-cultural variation" (p. 17).

Despite Ohnuki-Tierney's commitment to study Japanese culture historically, she abuses elementary precautions observed by historians. We learn, for example, that the medieval period was one of flexibility and mobility during which Japan was in motion at home and abroad, as suggested by "reestablished trade and cultural contacts with China in about 1342," but that the "dynamic tendency was soon overtaken by the opposite tendency . . . during the Early Modern period" (p. 15). Between 1342 and the "Early Modern" period (1603–1868), too much time elapsed to allow the facile use of the adverb "soon." A complaint is likewise in order about both the anachronism and the factual error involved in the assertion that "[Tokugawa] Ieyasu seized control of the entire nation"

(p. 13). And it is unfortunate that the editing process did not catch the misspelling of Sir George Sansom's surname everywhere it appears.

Students of Japanese ritual and Japanese attitudes toward animals as symbols or models of human behavior will find much of interest in this book. The author asserts a refreshing concern for the historical dimension of cultural performance. The final product, however, suffers from serious flaws as a work of history and notably fails to live up to the claim that it contributes to historical theory.

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JAHYUN KIM HABOUSH. *A Heritage of Kings: One Man's Monarchy in the Confucian World*. (Studies in Oriental Culture, number 21.) New York: Columbia University Press. 1988. Pp. xiv, 327. \$35.00.

King Yǒngjo (r. 1724–76) epitomizes a Confucian ruler of the late Yi Chosŏn dynasty who achieved the delicate balance of bureaucracy and aristocracy. He ruled Korea when the *yangban* ruling class reached the pinnacle of ideological, political, and social success. Yǒngjo's Confucian kingship was surely a reflection of the dynamic political, social, and cultural realities of eighteenth-century Korea. JaHyun Kim Haboush should be credited for her stimulating introduction to the Western world of this renowned Confucian ruler and his pursuit of sage kingship.

The book is supported by Haboush's extensive use of primary sources, both dynastic records and literary collections, and includes superb translations of the accounts of the last days of Prince Sado in the *Veritable Record (sillok)* and Yi Kwanghyon's diary as well as a historiographical introduction of five key sources. Through a skillful use of interdisciplinary analyses, namely, literary-intellectual, social-political, and psychological, Haboush attempts to understand Yǒngjo's practice and his ideas of kingship, politics, and society. For the comparative insights included, the book should be a recommended reading for students of Korea and of great interest to students of Confucian states and societies.

The first two chapters provide a historical background of Yǒngjo's quest for sage kingship and his technique of "image building" (p. 4). The author postulates that, after the fall of Ming China, the Korean elite and Yǒngjo, in particular, came to share a "new sense of mission" as "the sole custodians of civilization as well as the nation" (pp. 24–25). Haboush asserts that Yǒngjo cultivated his image as "carrier of the dynastic mission, chief priest, and civilizer" (p. 4) and used the rhetoric of the Confucian throne for monarchical authority. She observes that Yǒngjo also used the rituals and symbols, including the acts of penance and abstinence, so effectively that he contained bureaucratic opposition and found a way to appease "people"

(p. 104). What the author does not elaborate here is the underlying fact that the king's perception and action as a custodian of the Confucian civilization was an echo of the intellectual ethos of his contemporary *yangban* bureaucrats and aristocrats. She also informs us that Yǒngjo's affinity with the people in the street had something to do with the reputed origins of his mother, Lady Ch'oe, who was a secondary consort, "a concubine forever barred from the royal pantheon" (p. 54). Citing an "oral legend and an unofficial account" (p. 57), the author states that Lady Ch'oe had been a *musuri*, the lowest type of slave. In view of the lineage-conscious society of the eighteenth century, this is an extraordinary statement, which I find difficult to support without further evidence.

In chapters 3 and 4, the author discusses Yǒngjo's policies for the people and faction-minded court bureaucrats. In my view, chapter 3 is the most controversial, if not the weakest, part of the book. It is unfortunate that the author has accepted without question the theories of Shikata Hiroshi and Chōng Sōkchong about the eighteenth-century Korean society. Basically Haboush argues that there was an increasing number of "new *yangban*" because economic wealth was the decisive factor in upward social mobility and even "well-to-do peasants" could upgrade their social status, "bearing *yangban* privileges" (p. 89), by the purchase of titles, forged genealogies, and bribes. The author points out that Yǒngjo gained the support of those with "undistinguished rural background" and "undistinguished familial connections" (p. 93). But it is wrong to imply that they were primarily commoners or a "newly favored group" (p. 92). By Yǒngjo's time a majority of the local *yangban* families had many generations of undistinguished rural familial backgrounds. Similarly, the author makes a dubious distinction between the bureaucrats and *yangban* and seems to share the perception that the *yangban* community is above and outside of the "people" or "population at large" (p. 114). Yǒngjo's affinity with the "people," however, and his use of "popular opinion" cannot be disassociated from the local *yangban* or Confucian scholar-farmers who were the opinion makers of his time (p. 106).

Did Yǒngjo achieve sage kingship and practice autocracy? Haboush thinks that Yǒngjo became autocratic after 1740. He presided over the faction-ridden court, but he did not neglect it. Yǒngjo's "autocracy" was not institutionalized as in Qing China during the reigns of his contemporaries, such as emperors Yungjeng and Qienlung. Rather, it was a personalized autocracy by a learned Confucian ruler who knew how to deal with his political opposition. The Korean monarchs never succeeded in securing the throne beyond and above the influence of the bureaucratic *yangban* families in the countryside. Still, Haboush provides us in the last chapter with a brilliant psychoanalysis of Yǒngjo in his act of filicide. He was "a father to his people, and yet killed his own son," Regent Sado, the Prince of Mournful Thoughts (p. 167). The book is a

welcome addition to Korean studies and studies of Confucian states and societies. We need more monographs of this kind in Korean history; it compels us to question many assumptions and generalizations on the nature of the Yi Chosŏn state and society.

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NICHOLAS B. DIRKS. *The Hollow Crown: Ethnohistory of an Indian Kingdom*. (Cambridge South Asian Studies, number 39.) New York: Cambridge University Press. 1987. Pp. xxix, 458. \$49.50.

In this lengthy volume, based on a detailed study of the princely state of Pudukkottai, Nicholas B. Dirks provides us with what he calls an "ethnohistory" of a South Indian "little kingdom" (p. 5) from its late medieval origins to its transformation and ultimate collapse under the impact of British colonialism. In itself, the history of Pudukkottai, which at twelve hundred square miles is smaller than the average Indian district, is of little interest. What gives Dirks's work its importance is his attempt to use that state's social and political relations as "modal forms" (p. 405) for the analysis of India in the old regime and, to some degree, for the working of the British princely state system as well. Dirks further sees his study as contributing to a reassessment of the larger analytical categories, in theory and history alike, of comparative sociology.

The strength of the work is Dirks's fierce determination to be both historian and anthropologist. Built at once on a thorough mining of the archives of the Pudukkottai state and extended field work in the region, this study represents a model of the style of research associated with the burgeoning field of ethnohistory. No one who intends to undertake this kind of local historical study can afford to ignore this book. Yet one sometimes has the feeling that two books, one history and one anthropology, exist uneasily side by side in the pages of this volume. Of the four major sections of the work (apart from the theoretical introduction and conclusion), three, comprising nearly three hundred pages, are an examination of the rise and subsequent fall of the Pudukkottai state, while part 4, placed between the accounts of the eighteenth century and the colonial period, contains an ethnographic survey of contemporary social relations in the Pudukkottai area. Although those differing perspectives are meant to enrich our understanding of the subject, the transitions between them abruptly interrupt the continuity of the narrative, and they entail a substantial amount of repetition, especially in the account of the royal Kallar family. The detail in the historical portions, and above all in the ethnographic section, is so extensive that at times it nearly overwhelms the argument. A more tightly integrated format and more concise presentation of the data would have greatly improved the book.

Dirks's discussion of the "little kingdom" of the

precolonial era, although built on the recent work of such scholars of South India as Arjun Appadurai and Burton Stein, is original, sophisticated, and compelling. At its heart is an insistence that caste was not, as Louis Dumont would have it, defined by its own autonomous logic of purity and pollution based in religion but was rather embedded in a political context shaped by royal authority and the award of honors. Royal gifts, as part of a system of exchange and redistribution, articulated, in Dirks's view, a "political community" that was centered about the king and that lay at the heart of Indian society (pp. 130–31). More generally, he argues, religion and politics, caste and kinship all came together in a "totalistic social formation" ordered by relative proximity not to the person of the king but to the "principles of honor, status, and order embodied by the king" (p. 284). Although one may question whether the notion of the royal gift can bear so much explanatory weight—surely the social order was constituted in other ways as well—Dirks's focus on the king restores to studies of South India, preoccupied for the last several years with the temple and with segmented local communities, a necessary balance.

Dirks's account of the transformation of the political order under the British is argued with a rich subtlety informed by a wry irony. The book is worth having for that portion alone. What gave the princely state its distinctive character, in Dirks's view, was the persistence of the forms of the "little kingdom," above all the award of honors, in a system that sought to freeze social and political relations yet inevitably detached them from their previous significance. The result was a caricature of the old order, a "theatre state" (p. 384) in which kingship became increasingly the "hollow crown" of the book's title. In the end the rajah whom the British had trained as a "model" prince refused to play the game, married a white woman (to the horror of the Indian government), and went off to Australia!

So ambitious an undertaking as Dirks's book is inevitably not wholly successful. Yet it sets a high standard for the study of Indian society and opens up a number of new and suggestive avenues for our understanding of India's past.

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JOHN D. ROGERS. *Crime, Justice, and Society in Colonial Sri Lanka*. (London Studies on South Asia, number 5.) Riverdale, Md.: Riverdale and Curzon, London. 1987. Pp. x, 271. \$37.00.

In this ambitious book, John D. Rogers focuses on "several themes in the history of crime in colonial Sri Lanka, including the effectiveness of the administration of law and order, the relationship between the expansion of the market economy and crime, and the relative importance of unique cultural factors and broad forces of social change" (p. 243). The coverage,

however, is better for the period between 1865 and 1905 than for the entire British era (1796–1948) and for Sinhala Sri Lanka than for the predominantly Tamil-speaking areas.

An opening chapter offers a brief but useful survey of the politics, economy, and society of colonial Sri Lanka; chapter 2 is equally broad and sweeping in filling in the background on the administration of law and order. A familiar picture emerges here of the fits and starts with which an alien legal system gained a place of sorts in the indigenous culture. Less compelling, however, is the contention that the system “was soon adapted and accepted as an integral social institution by the mass of the people” (p. 40). More people may have been involved in civil and criminal cases in the nineteenth century than there were adult males in the entire population, but that statistic is not sufficient to establish the “immense popularity” (p. 40) of the courts. Certainly such numbers are not enough without a more thorough study of the social and political uses of the judicial system and of popular *mentalité* regarding that system.

A tendency to deemphasize the social and cultural context of crime and to rely on aggregate data may render some of Rogers's findings questionable, such as his surprising conclusions in chapter 6 regarding the economic and social backgrounds of offenders. If other investigators are, as the author asserts, guilty of readily accepting a causal relationship between “culture conflict” (p. 69) and the workings of the legal system, Rogers errs in downplaying that connection.

The most provocative and detailed sections of the book are chapters 3, 4, and 5, which look specifically at cattle stealing, homicide, and riots and disturbances as “case studies of crimes against property, the person and the state” (p. 1). The chapter on cattle stealing exemplifies the strengths and weaknesses of the work. However short it falls of meeting its stated objective of illuminating “many aspects of rural life, including the effectiveness of the colonial state in asserting its authority, the impact of changes in land use on village society, and the importance of geographical variations when tracing economic and social trends” (p. 82), its conclusions relating cattle theft to the changing economy of the island (from rice to plantation agriculture) are not without significant insights.

Too much is attempted here, but the book's shortcomings partly reflect the incipient state of the art in the history of colonial crime. Henceforth, historical crime watchers of Sri Lanka will have to begin with this important investigation.

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ALAN FROST. *Arthur Phillip, 1738–1814: His Voyaging*. New York: Oxford University Press. 1987. Pp. xii, 320. \$48.00.

Australians recently celebrated their bicentennial, the establishment of New South Wales at Sydney Cove in January 1788. The founder of that colony, its first governor, and commander of the First Fleet was Arthur Phillip, an officer of the Royal Navy. The First Fleet brought fifteen hundred people, half of them convicts, from Britain for the initial settlement. The fleet's health record en route (May 1787–January 1788) was remarkably good (forty-eight died). Site selection proved to be difficult. The entire entourage remained isolated, what Alan Frost appropriately describes as “out of the world” (chap. 4), until the arrival of the Second Fleet in June 1790. A government-sponsored resupply ship was wrecked. Nevertheless, a viable colony of four thousand people was thriving when Phillip departed for home in December 1792.

This panegyric, in which Phillip is compared to Ulysses, presents “a panorama of his life and times” (p. xi). In the manner of Whig history, all of the salient events of the life of Phillip are made to appear as preparation for the complications and vicissitudes of planting an antipodal penal colony. He was an orphan from Greenwich Hospital School, an apprentice whaler and commercial seaman, a midshipman and junior officer in the Royal Navy, a senior officer in the Portuguese navy, an intelligence agent, a war veteran (of the battle of Minorca and the siege of Havana, among others), a surveyor and explorer, and a commander. He voyaged to seven continents. After his governorship he returned to the navy and retired as an admiral. There were intervals of poor health, half pay, two marriages with no children, and periods when he is “lost” to history (pp. 32, 54). Frost admits that there are gaps in the record through which he has diligently searched. He has gathered as much information as possible. On occasion he speculates with plausible explanations, for example, that Phillip established a penal colony for the Portuguese in the 1770s and that he operated as a secret intelligence agent for Britain.

Frost reviews historiographical controversies, for example, the nature, motivation, and rationale for assembling the First Fleet and establishing a colony in Terra Australis Incognita. As might be expected from the author of *Convicts and Empire* (1980), Frost prefers the explanation of imperial strategic expansion as the “real motivation” of the government of William Pitt in the 1780s. The penal colony—the dumping ground thesis—takes second place in his view. Frost sees the role of Phillip as decisive, even as influencing the future shape and development of Australian government, for example, the dominance of central authority and egalitarianism.

This book is highly attractive with a luxurious layout, fabulous illustrations, numerous maps and charts, and a unique technique, repeated seven times, of progressively displaying the extensive voyaging of Phillip with two-page world maps. It is intensively researched with explanatory notes on sources. Its weaknesses include lapses into speculation, cumbersome organization, and

careless use of naval terminology, for example, "battle-ships" (pp. 26, 132), and incorrect ranks.

This work is an excellent addition to the recent series of outstanding publications on exploration and scientific endeavors, which includes Rudiger Joppien's *Art of Captain Cook's Voyages* (3 vols., 1985–88), Edward Cree's *Cree Journals* (1981), and Richard Keynes's *"Beagle" Record* (1979).

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FRANK FROST. *Australia's War in Vietnam*. Boston: Allen and Unwin. 1987. Pp. xii, 211. \$34.95.

The book under review is one of an increasing number of books dealing with the role of third countries in the Vietnam War. It is a subject of some significance. Although the actual number of Australian troops serving in South Vietnam was quite limited, relations between the United States and Australia were particularly close, and, as Frank Frost shows, on a number of occasions Canberra's views may have played a part in shaping U.S. policy in the war.

The origins of Australian concern about Vietnam go back to the early 1950s, when the brunt of the battle against the communist-led Vietminh was borne by the French. Australian officials and the public in general had been shaken by the fall of China to communism and the danger of falling dominoes in Southeast Asia, and Australia followed the U.S. lead in providing diplomatic and military assistance to the French-sponsored government of Bao Dai.

Direct intervention in Vietnam did not take place until the mid-1960s, however, with the rapid rise of the revolutionary movement against the regime of Ngo Dinh Diem in the South. Like John F. Kennedy's administration in the U.S., the government of Prime Minister Robert Menzies saw the conflict essentially as an invasion from the North. Fears had already been aroused in Canberra by the actions of President Achmed Sukarno in Indonesia, whose policy of *konfrontasi* with Malaysia and close relationship with China raised the specter of growing instability throughout Southeast Asia.

The Australian government was therefore receptive when, in 1964, Washington appealed to its allies to "show the flag" as a gesture of support against the forces of communism in South Vietnam. In fact, some Australian officials were more militant than their counterparts in Lyndon Johnson's administration and viewed the offer of a battalion of Australian combat troops as a means of providing moral support and encouragement to the United States in maintaining an active presence in Southeast Asia.

During the next several years, a total of 46,852 Australians served in South Vietnam, reaching a peak level of 8,300 in the late 1960s. At first, Australian forces were assigned to U.S. units, but that experiment was abandoned because of operational disagreements.

They were eventually given responsibility for the province of Phuoc Tuy, near Saigon. Australian units encountered some of the same problems faced by U.S. forces—lack of clear purpose, difficult relations with South Vietnamese troops, and an inability to achieve long-lasting success. But, in Frost's account, there were some key differences. Australian troops were better disciplined, more motivated, and less inclined to use drugs than were Americans. In fact, some correspondents were critical of the "diggers" for their uncritical acceptance of the stated objectives of the war.

Australian units began to be withdrawn from Vietnam after the Tet offensive, and the last unit departed in December 1971. Today, many Australians question the meaning of the war, while their government has begun to reassess the validity of its close alliance with the United States. Frost shares many of these views and concludes that, in attempting to provide loyal support to an ally, Australian policy makers failed to discern the underlying reality of the civil struggle in Vietnam.

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DAVID HANLON. *Upon a Stone Altar: A History of the Island of Pohnpei to 1890*. (Pacific Islands Monograph Series, number 5.) Honolulu: University of Hawaii Press, for the Center for Pacific Islands Studies, School of Hawaiian, Asian, and Pacific Studies, University of Hawaii. 1988. Pp. xxviii, 320. \$32.00.

This excellent book treats the history of the island of Pohnpei in the eastern Carolines of Micronesia from precontact times to the declining years of the Spanish colonial period. David Hanlon treats the opening of Pohnpei—formerly known in Western usage as Ponape—to the European world by explorers, traders, whalers, and missionaries and the early attempts at colonization by the Spanish. It is the first such book of its kind—a deliberate history of the island—that reflects respectable and meticulous scholarship.

But, in addition to being a fine work, its appearance brings up at least three historiographical issues much debated over the last decade that will continue to be discussed over the coming decade: the worthiness of studying such small places among the Pacific islands, the consideration of subjects from the islanders' points of view, and the use of traditional oral sources in writing and interpretation. Hanlon's own background as a Peace Corps volunteer on Pohnpei and his mastery of the Pohnpeian language, followed by several years as a member of the faculty at the Community College of Micronesia at Pohnpei and his one-year stint as a researcher for the Trust Territory Office of Historic Preservation, prepared him singularly to research and write this work.

It has been only quite recently that Western scholars have become interested in seriously studying the Pacific islands. The historiographic work and inspiration of the late J. W. Davidson of the Australian National

University prompted a number of truly excellent studies and practitioners. None, however, has the verve, scope, and sensitivity that Hanlon and his work possess. He not only uses Pohnpeian oral sources but also employs them with respect, style, and poignancy, which renders the book more than an excellent deliberate history. His study is a model of the appropriate usage and integration of traditional oral sources into a Western-style documented historical work.

The first chapter in this book relates the Pohnpeian version of the prehistory of the island, the story of the Nan Madol ruins, and the beginning social organization of the local polity. So well executed is Hanlon's use of Pohnpeian legends, stories, and folklore that many Western historians skeptical of such usage will find their usual criticisms of traditional oral historiography vitiated. Finally, Hanlon goes as far or farther than any Pacific historian from the West in representing his interpretations of the historical pageant of Pohnpei in terms of the way that the Pohnpeians experienced events.

The only criticisms that might reasonably be made concern Hanlon's reasons for concluding his study in the year 1890. He might have carried through another twenty-four years to the end of the German administration in 1914 or at least another nine years to the end of the Spanish period in 1899. He offers three justifications for not doing so. First and most important is that by 1890 the Pohnpeians had demonstrated that they had withstood the various waves of foreigners without losing their identities as Pohnpeians. Second, and strongly related to the first, is that the Pohnpeians would not permit the Spanish to dominate them, although they did use some European-introduced resources. Third, American Protestant missionaries, who arrived in the 1850s and who were a strong influence on the people of Pohnpei, left the island in 1890 for a ten-year period.

For readers unfamiliar with the islands, there is an excellent guide to Pohnpeian spelling and pronunciation. The maps and photos, which are especially important in historical works of islands, are excellent and enhance the reader's understanding of the text. Finally, the notes and bibliography are thorough and will provide great assistance to those who wish to pursue island history further.

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CLAUDIA KNAPMAN. *White Women in Fiji, 1835-1930: The Ruin of Empire?* Boston: Allen and Unwin. 1986. Pp. xiv, 226. \$16.95.

Claudia Knapman has undertaken a study of white women in Fiji that should (but probably will not) put to rest the clichés and misunderstandings about the roles they assumed in colonial societies. Fiji was never a society of European settlers. Nevertheless, the impact of European influence has been, and continues to be,

inescapable. Introduced diseases, political decisions to import Indian laborers, the growth of large colonial enterprises and their company towns, all had their effects on Fijian society. Throughout the nineteenth and twentieth centuries, social and ethnic divisions became more marked as British categories were superimposed on the already stratified Fijian social order. Emerging divisions among Indians, Fijians, and whites were cast in racial terms, and women, so it has been argued, were the principal force behind the promotion of racism. Motivated both by sexual jealousy and by their own oppression as victims of gender stratification, they supposedly escalated racial tensions by exercising whatever superiority they could muster over native peoples. That rather unpleasant portrait of women has been painted regularly in studies of colonial societies, each new study reinforcing a negative view of women that is, at this point, almost axiomatic.

Knapman is convinced, and convincingly demonstrates, that male bias rather than women's actual activities is responsible for the historical role assigned to women. Her outrage is clear. "Such perceptions tell us more about the values of the authors, and the pervasive values of our society, than about the attitudes and behaviours of men and women in multiracial colonial societies. Such unprofessionalism and lack of scrutiny would scarcely be tolerated if the issue impinged on male self-respect and identity" (p. 15). Moreover, she points out the paradox that results when women are denied autonomy but nevertheless held responsible for deteriorating race relations. "Why are women (whether 'idle' or 'moralistic') regarded as irrelevant to the main historical themes and events and at the same time seen as significant in the vital area of race relations" (p. 16)?

Indeed, women are hard to find in the historical record. Before Knapman's study, only limited published material was available. She has taken advantage of an abundance of unpublished materials to document the difficulties encountered by the wives of missionaries and early settlers in Fiji. Furthermore, by analyzing a century of colonial experience, she is able to document the increasing stratification of both Fijian society and its settler component. Her description of the development and consequences of social differentiation is one of the strongest aspects of the book.

She also addresses the ideology that formed the basis of racial and sexual identity. She argues that both gender and race are products of the dualistic tradition in Western thought. Although there can be no doubt that women and blacks have both been victims of patriarchal institutions, an appeal to Western ideology is far too general to be of much use here. Her account is strong when she discusses particulars; her analysis of the images of white and black among Europeans in Fiji is clear and lucid. But her more general theoretical discussion raises, rather than answers, questions. A major strength of her book is the exploration of a specific social and cultural context. By relying on a theoretical position derived from an entirely different

set of cultural circumstances, she weakens her argument. One of her sources is Eldridge Cleaver. Are the writings of a twentieth-century American black man really able to illuminate the motivations of white men on the margins of a fading British empire a century earlier? The notion that oppression is everywhere the same, that it results from dualism in Western thinking, begs rather than addresses the question of the Fijian case. Perhaps what she is really arguing is that the British, colonizing in Africa, the Pacific, and North America, were everywhere the same. But Western thought is too flimsy an explanatory edifice on which to hang centuries of oppression in different cultural contexts. Surprisingly, she seems to accept an analysis based on feelings of sexual dominance and submission. In refuting a psychoanalytic interpretation, she nevertheless only seems to add to its ostensible credibility. This is puzzling because her rich material would not appear to justify such an emphasis. For example, she writes, "Both white women and black men were defined by contrast and opposition to the white man. The European woman was her father's, or her husband's property, conceptually a minor and his ward. The black labourer was similarly his master's property, and even in enlightened thought, the black man was a minor relative to the white. Therefore at a deeper less conscious level the prospect of the liaison of his white wife and his black servant was not only impossible, and a betrayal, but also incestuous" (p. 135). Her last chapters do, however, return to a sounder theoretical perspective. She documents her main thesis that a coincidence in time between the growth of racial antagonism and the arrival of European women has been understood as a causal connection. If anything, bad relations between Fijians and Europeans were exacerbated by men: sugar planters who were looking for land, labor, and appropriate social conditions, the owners of sugar companies who mandated social and ethnic divisions. It is ridiculous to blame women, even those who shared their husbands' economic and social aspirations, when it is so clearly men who imperiously attempted to commandeer Fiji's resources.

This book is valuable reading for anyone interested in colonial history or for those interested in the roles taken by women in colonial enterprises. I only wish that Knapman had drawn out the implications of her conclusions. For ultimately women were not responsible for deteriorating race relations, because in fact they were responsible for very little that took place in colonial Fiji.

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NELSON W. ALDRICH, JR. *Old Money: The Mythology of America's Upper Class*. New York: Alfred A. Knopf. 1988. Pp. xvi, 309. \$19.95.

Nelson W. Aldrich, Jr.'s critical appreciation of "Old Money" enacts the virtues the author claims for the

social class at its best. The prose is graceful, the authorial voice is self-assured, and the observations are trenchant—the consequence of wide learning, unbelabored. Above all, the text is an act of *pietas*, or reverence for ancestry, performed by an erstwhile black sheep and intellectual son of Old Money. The author's subject is Old Money understood as a distinct social class constituted by the imaginative acts of its own members and others. The chief characteristic of its members is that they perceive their wealth as tangible property, as estates with lineages, enduring through time. From this perception and the attendant obligation to preserve patrimonies follow a cluster of virtues, among them a lively sense of history, a feeling of participation in a collective destiny, and an expansive definition of social responsibility, conceived of on the model of trusteeship and including stewardship over the nation's patrimony—its wilderness and its art.

The author also creates a continuous argument between Old Money and its negative other, "New Money." The entrepreneurial imagination conceives of money as abstract and fluid, as giving rise to an endless field of possibilities. New Money's main virtue is its insistence on the generative, productive power of money; its vices are a foreshortened sense of time, a lack of social conscience, and an egoism disguising a precarious sense of self. What Old Money has most to fear from the dominant entrepreneurial attitude is not envy but indifference, an indifference that would render the class invisible. New Money's most effective retort to F. Scott Fitzgerald's claim that the rich are "different" is Ernest Hemingway's belittling and probably borrowed reply, "Yes, they have more money" (p. 265). Aldrich's main task is to argue that Old Money is different, sponsoring values that we eliminate at our peril.

A secondary danger is cooptation. Ralph Lauren's effective advertising is symptomatic of the New Money claim that one can simply buy Old Money taste. George Bush's embrace of the Reagan administration's *laissez-faire* policies and his representation of self as a modest, average guy during his presidential campaign are cooptation internalized; Bush is a traitor to his class. (Aldrich's book was published before the author could evaluate Bush's stance as guardian of the environment—a partial reversion to his Old Money roots.)

Scholars who specialize in the study of elites may view the book as primary data. One could not ask for a more articulate and observant informant. But the book is astute sociology in its own right. Aldrich's analysis of the institutions that tutor children of Old Money in the virtues of the class (and confer Old Money status on deserving aspirants) is especially insightful. Academics will appreciate, in particular, his account of the way in which Harvard and its equivalents reconcile their duty to the children of the rich with competitive, meritocratic principles. He also takes on academic guild members occasionally. E. Digby Baltzell, for instance, was wrong when he called the author's great-grandfather, Nelson W. Aldrich, a Brahmin. The first Aldrich successfully established his family estate on Provi-

dence's College Hill via fortunes accumulated as a corrupt senator from Rhode Island and as an intimate of J. P. Morgan, but he was no more than an arriviste during his lifetime.

Scholars may also wish to quibble about minor matters. For example, Aldrich's portrayal of the young Thomas Jefferson as the early embodiment of the entrepreneurial spirit will not convince those who prefer a more republican, less liberal Jefferson. More seriously, defining a social class primarily by its subjective representation makes for difficulty in delimiting its boundaries. According to Aldrich, the "aging process" conferring Old Money status and normally beginning with a founder's children "may run so fast and powerfully that it pulls the parents along with it" (p. 30). Although this "elastic" property of time (p. 30) may express a truth about America's relatively open social landscape, it also allows Aldrich to bestow the label Old Money on figures such as John F. Kennedy. It becomes difficult to know whom, among the very rich, to exclude provided they make the correct aesthetic choices.

Finally, make no mistake about it. This book is a brief on behalf of inherited wealth. The defense is effective partly because it includes a disarming, clear-sighted analysis of the defects typical of the class. But there is also a sleight of hand. The defense works because the only alternative is New Money. Who would not prefer Old Money taste and its stewardship to the empty vulgarity and selfish depredations of New Money? But since we are operating in the realm of the social imagination, perhaps Old Money should be evaluated against other, more egalitarian visions of responsibility and welfare.

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JOHN R. SUTTON. *Stubborn Children: Controlling Delinquency in the United States, 1640-1981*. (Medicine and Society.) Berkeley and Los Angeles: University of California Press. 1988. Pp. xii, 299. \$35.00.

This book traces American society's legal responses to children's misbehavior and advances the thesis that reforms in the juvenile justice system merely changed the administrative structure without affecting an informal decision-making process outside the jurisdiction of the courts. John R. Sutton regards the "stubborn child" law passed in Massachusetts in 1646 as the first significant step in the process of creating the juvenile justice system in the United States, but he sees the development in the early nineteenth century of a series of institutions designed to remove children from adult prisons as most important. The law establishing those institutions—called "houses of refuge"—laid down the principle of separate institutions for juveniles, gave the refuges (and, by extension, the juvenile justice system) jurisdiction over noncriminal children, and deprived children of the right of court supervision of the process of commitment. Thus, Sutton argues, the

legislation creating the houses of refuge, not that creating the first juvenile court in 1899, represented the primary innovation in American juvenile justice law.

Another significant contribution of Sutton's book is that it provides a succinct and persuasive account of major developments in the juvenile justice system in the 1960s and 1970s. Two Supreme Court cases, *Kent v. U.S.* (1966) and *In re Gault* (1967), required juvenile courts to guarantee certain procedural rights to juveniles accused of crimes. Those cases appeared to improve the situation for children inside the system, but the federal Juvenile Justice and Delinquency Prevention Act of 1974 encouraged the juvenile justice system to develop "diversion" programs for "status offenders" (children whose actions or situations would not bring them before the courts if they were adults). The law had the effect of removing many children from the jurisdiction of the juvenile court, while retaining the informal decision-making powers of the system as a whole.

This book is an important addition to the literature on the history of juvenile justice in the United States. Sutton argues persuasively that the legal importance of the juvenile court has been overrated, and he advances a convincing thesis that reform of the juvenile justice system has had little practical effect. Sutton suggests that there needs to be more research on the people who have served in the system or who have been its clients. Thus, historical analysis of the day-to-day workings of the system would appear to be the next project. Current staff in the juvenile justice system can profit from the implications of this book, and historians of social welfare, children, and criminal justice will want to make use of its findings.

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JERALD C. BRAUER, EDITOR. *The Lively Experiment Continued*. Macon, Ga.: Mercer University Press. 1987. Pp. xix, 250. Cloth \$39.95, paper \$18.95.

In spite of its black cover (a design artist's idea of a joke?), *The Lively Experiment Continued* is a lively book. This collection of essays, a festschrift to honor American religious historian Sidney E. Mead, takes titular inspiration from Mead's classic collection of essays, *The Lively Experiment* (1963). Mead's title referred not to the contents of his own book (although a strong case can be made for the appropriateness of that description) but to the continuing condition of Christianity in America. The title of the new volume, however, has more than one meaning: it includes both religion in America and the efforts of the contributors to the volume to understand American religion with reference to Mead's work. The title also suggests a third—and, in the end, the most important—concern of the volume: a historiographical assessment of the contribution of Mead himself as scholar-teacher.

Festschrifts, of course, can easily be catchalls into which the great or nearly great (or at least the solidly respectable) toss whatever they do not wish to publish elsewhere. Thus, festschrifts can be only minimally related to the person whose work they ostensibly seek to honor. Such is not the case for this volume. The book has structural unity and thematic consistency. The careful introduction by Jerald C. Brauer provides context, background, and cues to what follows. The first section (essays by Brauer, Martin E. Marty, and J. Ronald Engel) assesses Mead's contribution as historian, teacher, and theologian. The second (essays by Winthrop S. Hudson, Edwin S. Gaustad, and William G. McLoughlin), in the tradition of Mead's pervasive and "big picture" emphasis on the "religion of the Republic" (his term for what has come to be called "civil religion"), explores themes concerning liberty and its limits—especially for Indians—in the revolutionary and early national periods.

The third section, with contributions by some of Mead's students (Richard T. Hughes, Ronald B. Flowers, Jo Ann Manfra, Robert T. Handy), applies his interpretive categories to aspects of American religious life that he never extensively studied (Mormonism, interpretive norms for the contemporary Supreme Court, Roman Catholicism, and Canadian religion). A final essay in the third section by Mead himself ("Re-interpretation in American Church History") gives him the last word—one that is a reflection on his own methodological stance as much as it is an interpretation of the history of American religion.

Some of the essays are better than others as discrete and "finished" entities, and some advance fresh and significant interpretations more successfully than do others. This is to be expected. But the strength and the "liveliness" of the volume lie not so much in the quality of individual contributions (although overall it is impressive) as in the involvement with Mead's concerns that informs the whole. The overarching religion of the republic in tension with the divisiveness of the sects, the realities of pluralism in conflict with the wish (of some) to escape it, the demands of history in the context of an ideology of "historylessness," the idealism of the American religious experiment in the face of its historical failures—these interpretive themes provide motifs that overlap and texture individual essays. Indeed, in the tradition of Mead, this volume stretches the term "Christianity" to encompass religion in general and even the cultural dynamics that impinge on religion in America. Thus, the chief tribute the book pays to Mead is to show the viability of doing history in touch with his categories and concerns—even when, as especially in one essay, the problems of his reading form the agenda. In short, what the volume does best is to introduce readers to the interpretive vision of Sidney Mead.

CATHERINE L. ALBANESE
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ROBERT W. JENSON. *America's Theologian: A Recommendation of Jonathan Edwards*. New York: Oxford University Press. 1988. Pp. ix, 224. \$26.00.

More than a quarter of a century ago, Sydney Ahlstrom lamented that Jonathan Edwards had been carved into a number of convenient pieces. Neither his contemporaries nor his theological heirs nor scholars of our own time, Ahlstrom complained, had been able to grasp Edwards's vision in its wholeness. This volume by Robert W. Jenson may not "put Humpty-Dumpty together again," but it makes an admirable attempt to reconstruct the integrity of Edwards's theology and then to give it a contemporary meaning.

This is not biography in any sense; the details of Edwards's life are provided only in the bare essentials. Jenson's focus is the unity of Edwards's ideas across his life rather than their development through his lifetime. He makes no attempt to illuminate the theological evolution of New England's theology, though some specific issues, christology for example, are clarified in passing. More than any other volume on Edwards with which I am familiar, this is a theological analysis with a theological and didactic purpose.

Jenson disclaims any originality, but the force of the analysis (and hence its originality) stems from the author's perspective. He understands Edwards through the sympathetic eyes of his own faith. For his insights into the mind of Edwards, he depends almost exclusively on the Yale edition of *The Works of Jonathan Edwards* (1957–) and the unpublished transcripts of the "Miscellanies." There is a freshness in this, and the few intellectual skirmishes in which Jenson engages are relegated to the notes.

It takes several chapters before the title that Jenson chose reveals its full meaning. What had seemed innocently descriptive takes on a proscriptive meaning. Edwards is, in Jenson's view, not only America's greatest theologian but also the one theologian who can speak to the ills of contemporary society. "Recommendation" in this sense is not simply a positive evaluation, it is an endorsement of Edwards's basic Reformed theology as a curative for our liberal malaise. Jenson sees America as the compliant victim of the revolutionary Enlightenment in which it was born. Edwards becomes the contemporary hope because he was the individual who met the Enlightenment head-on, confronted its attack on authority, and directed a devastating critique at its religious accommodators. To the question, posed in the introduction, whether Edwards was not only right then but also right now (p. viii), Jenson answers with a resounding yes.

Other scholars of Edwards have touched some of these themes. Paul Ramsey has remarked that modern liberal theology "might have gained firmness and substance if it had stopped to answer Edwards" (*Freedom of the Will* [1957], p. 10), and Henry May has echoed a variation of that statement in *The Enlightenment in America* (1976). Jenson has used Edwards as a vehicle to write his own jeremiad. Editorial asides castigating the

emptiness and sterility of contemporary America are frequent, intrusive, and purposeful. They may well serve the purpose that Jenson bares in his title, but they will inevitably distract unsympathetic readers from his quite successful recovery of an integrated Edwardsean theology.

ROBERT POPE
State University of New York,
Buffalo

DANIEL W. BJORK. *William James: The Center of His Vision*. New York: Columbia University Press. 1988. Pp. xix, 338. \$35.00.

Daniel W. Bjork's brief, readable study ambitiously aims to revise biographical understanding of William James and to reveal the fundamental key to his thought. James's center, according to this study, lay in the lack of a center, in the "relational area between the fields—between art, natural science, medicine, aesthetics, psychology, and philosophy"—that interested him (p. xvii). This diffuse approach yields few new insights. At the end of Bjork's analysis, James's vision becomes a bit more centered in his late expositions on metaphysics that constituted the universe in relations between bits of pure experience. But the problem of which James ran afoul, the problem of how those relations were established, is scarcely mentioned. Readers interested in James's thought should consult the recent searching, critical studies *William James: His Life and Thought* (1986) by Gerald E. Myers and *William James* (1986) by Graham Bird.

Bjork is properly suspicious of the legend that James passed from neurasthenic suffering to enduring, vibrant mental health by a simple assertion of will after reading the French philosopher Charles-Bernard Renouvier. But Bjork's challenges to that legend are not strong efforts. As an explanation superior to "will" for James's recovery of health in 1870, Bjork suggests that life, to James, "was not really worth living except in the experience of creation, the acting out of his special genius" (p. 89). If this noble feeling characterized James, it characterized him during as well as after his suffering and can scarcely explain his recovery. Similarly, the latter part of the book promises a look at a James "essentially untouched by the optimistic rhetoric he had done so much to perpetuate" (p. 229) and even invokes a mention by James of Melville's Ahab. But from there on, there is little but description of James's decade-long, understandably desperate search for a cure for his chronic heart disease. The degree of interpretive license in this study may be gauged by the most likely answer to the rhetorical question whether the youthful James, accompanying Louis Agassiz to the Amazon, there first hit on the notion that the stream of consciousness "shaped the mind as the Paraná had shaped the channel and islands" (p. 66).

Thanks to Bjork's thorough mining of primary materials there are a few new bits of humanity to relieve

James's reputation of its spotless sacrosanctness. James regarded George Herbert Palmer, his colleague in philosophy at Harvard, as an "insulting brute" (p. 170). Suicide was the "only manly and moral" course for his alcoholic younger brother, Robertson James, but William condescendingly added that "Bob will ne'er do that, I'm sure" (p. 190). The sainted James was even capable of exasperation with his wife (p. 237). But these are tidbits. James remains personally mysterious. We still await the book that will well acquaint us with the most famous American philosopher rather than merely, if probably rightly, assert that, "despite his genuine humanity and remarkable intelligence, James was nonetheless a splendid New World example of self-centeredness cloaked in moralism" (p. 107).

JAMES HOOPES
Babson College

THOMAS R. DUNLAP. *Saving America's Wildlife*. Princeton: Princeton University Press. 1988. Pp. xvi, 222. \$24.95.

Do not be deterred by the thin appearance of this book—176 pages of text. The print is small, the pages are fairly large, and every word counts. This thoroughly researched, well-written history of the movement to save wild animals fills a vacuum. It is an excellent example of our discipline's value to other areas of endeavor.

Thomas R. Dunlap divides his study into three parts. Part I, "Foundations for a Wildlife Policy," begins with generalizations about the pioneers' approach to wildlife "as objects to be used and used up" (p. 5). In the mid-nineteenth century, hunting became a gentleman's sport. Simultaneously the concepts grew that some animals, usually ungulates that were prime hunting prey, were "good" animals and that the beasts that killed them were "bad" animals to be mercilessly destroyed; so, too, with birds of prey.

Those ideas softened somewhat in the public mind with the animal stories of Ernest Seton Thompson, Charles G. D. Roberts, and others. Their characters were anthropomorphic: the animals acted and thought like humans. An example of such "nature faker" writing was the story of the woodcock that made a cast of mud and straw for its broken leg. The naturalist John Burroughs and Theodore Roosevelt chided the authors for presenting such nonsense as truth.

Finally came government involvement with the creation of the United States Biological Survey. Soon the survey was involved in predator control. It, or the same agency with a succession of names, cooperated with ranchers to eliminate predators as well as ground squirrels and prairie dogs. People who questioned those policies founded their own organizations. In 1915 the Ecological Society of America was organized, and in 1919 the Society of American Mammalogists came into being. But, as Dunlap points out, they possessed "a set of ideas in search of an organizing

principle" (p. 45). Other groups advocated an end to feathered hats or the outlawing of leg-hold traps.

In part 2, "A Time of Transition," Dunlap traces side by side the progress of wildlife protection and the bureaucratic defense of predator policy into the 1960s. It is difficult for our generation to believe that intelligent, educated wildlife authorities could have failed to realize that the killing of predators would create many new problems. Partly as a result of the proliferation of the Kaibab deer, bureaucrats began to have second thoughts about the destruction of predators. But deer hunters have always resented cougars "killing our deer," and ranchers, especially sheepmen, have continued to advocate vigorous coyote control programs. Nor are those groups enthusiastic about the reintroduction of wolves. The public, however, educated by such books as Rachel Carson's *Silent Spring* (1962), Fairfield Osborn's *Our Plundered Planet* (1948), and Farley Mowat's *Never Cry Wolf* (1963), became insistent that changes favoring the predators be made.

Part 3, "New Ideas, New Action," takes the reader through the last quarter century. Chapter titles indicate the subject matter: "Ending the Poisoning," "Saving Species," and "Finding Equilibrium." The last chapter, in which Dunlap shows how the Ronald Reagan administration backtracked from much of the progress made just decades before, acquaints the reader with the situation as it exists today.

Dunlap states that his story is told "with emphasis on wolves and coyotes" (p. xi). The emphasis is hardly discernible. Discussion also includes prairie dogs, the black-footed ferret, and, to a lesser degree, bobcats, cougars (or mountain lions), and predatory birds; the saving of the whooping crane is also mentioned, as is the crusade for ducks and geese.

This book is a major contribution to conservation and wildlife-management literature. It is based on extensive use of primary sources and is essential background material for anyone involved in any way in the history of predator control and wildlife management.

RICHARD A. BARTLETT
EMERITUS
Florida State University

ROSEMARIE ZAGARRI. *The Politics of Size: Representation in the United States, 1776-1850*. Ithaca: Cornell University Press. 1987. Pp. x, 165. \$23.50.

As children many of us played "game of the states," a relatively pleasant way to memorize the list of American state capitals. Grown-up readers of this volume will know fully why that list contains so many obscure names and why it maddeningly seems to exclude the major cities, the real centers of power and population.

Our late eighteenth-century forebears had to work their way out of the conundrum created by their rejection of virtual representation in favor of actual representation. Actual representation might be possible for a small city-state, their intellectual legacy stated,

but how could it not deteriorate into tyranny or anarchy in territories of such vast size as the American states? The solution was to reject the metaphor of the body politic and to think instead of a circle. Find its true center, usually geographic in smaller states and demographic in larger ones, and actual representation would become equitable in a capital located on that spot. There were no sophisticated computers to map exactly the proper coordinates, and local political horse trading played its part. But the results were not random; state capitals were removed from coastal centers of power to new and obscure interior sites equally accessible, or inaccessible, to all. As it turned out, expectations that the new capitals would become major cities and cause the depopulation of coastal towns did not materialize, but that fact in no way lessens the importance of the debate to its historical participants.

The same division of large versus small states so well known for its appearance during the Constitutional Convention occurred repeatedly in the era during which the new republic was forged. Large states advocated proportional representation (in every case, after much debate, based primarily on population rather than geographic size or wealth); they quickly adopted district-based congressional electoral systems and consequently produced politically divided delegations; and they favored a numerically large House of Representatives to offset the unfair advantage to the small states in the Senate. The small states tended to retain corporate representation of established communities, usually towns or counties; they opted more often for general-ticket electoral systems, which in turn fostered politically unified delegations that did not adequately represent minority interests; and they fought to keep the House small. The two sides bickered after every census over fractions, and gradually expediency came to dominate intellectual rigor.

Particularly intriguing is Rosemarie Zagarrri's closing argument that the declining salience of the division between large and small states, especially after 1842, contributed to the political potency of the split between North and South. The point undoubtedly rests on too many technical considerations to find a major place in the vast and often polemical historiography of pre-Civil War politics, but even a well-earned footnote is a worthy accomplishment. Zagarrri writes in a modest and clear style and tells her story well. This slim volume is an exercise in human geography and politics that deserves a place on the American historian's library shelf.

RUDOLPH M. BELL
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New Brunswick

DAVID W. BRADY. *Critical Elections and Congressional Policy Making*. (Stanford Studies in the New Political History.) Stanford: Stanford University Press. 1988. Pp. xiv, 212. \$27.50.

David W. Brady's study of the U.S. House of Representatives offers the most ambitious test to date of the policy implications of party realignment. In so doing the author applies to one of the understudied institutions of the nation a perspective about change and continuity in the American polity that enjoys wide currency among historians and political scientists. Brady's approach epitomizes the theoretically anchored and statistically based methodology of quantitatively oriented political scientists. His research design combines a longitudinal format with massive amounts of electoral and legislative voting data, usually presented in aggregate form. The scale of the project is reminiscent of Richard F. Bensel's *Sectionalism and American Political Development* (1984), which provides another schematic pass at congressional history. Both volumes suffer from touches of monocausality. In Bensel's case the problem took form as a sectionally based economic determinism. With Brady, party is the dominant instrument of national policy making. Both works are provocative because their authors prefer generalizations cast as dichotomous contrasts to phrasing sensitive to complexities of politics.

Realignment theory posits that a few "critical" elections in the United States set the stage for major policy changes. Each electoral disjuncture cast up a new majority that combined a substantive mandate for governing with unified partisan control of America's triparted government (two legislative chambers and an executive). Those rare conjunctures resulted in "outpourings of new, comprehensive public policies" (p. 2). Subsequent popular approval of each new regime ("retrospective voting") ratified its policy innovations and thus institutionalized the political realignment. Brady's useful addendum to this evolving theory holds that the prerequisite for overcoming Congress's customary incremental approach to policy was mass turnover of congressional committee membership. New people forged new policy.

The empirical heart of the book appears in chapter-length examinations of the Civil War, the 1890s, and the New Deal—eras of realignment. Brady analyzes the swings in the partisan vote for congressional candidates by regions and specifies how those electoral fluctuations translated into the partisan redistribution of House seats. The author relies primarily on correlations between lawmakers' party affiliations and their scores on "scaled" collections of roll call votes to gauge the tenor of congressional policy making. Elevated levels of partisan conflict in legislative voting after a critical election are interpreted as evidence that a new majority party fulfilled its policy mandate. Case studies of the appropriations and the agricultural committees and an exploration of the connection between competitiveness in House districts and the partisan turnover of seats supplement the period surveys. Brady convincingly demonstrates that a large number of close election contests was a prerequisite for decisive alternation of party control of the House. The dominance of safe

seats in the present Congress, therefore, lessens the likelihood of an impending realignment.

Brady sees the veracity of realignment theory in these data. Fundamental policy changes were triggered by critical elections and were implemented by the new advantaged political party. His confidence in this conclusion is partially an artifact of his measurement strategy and partially the product of his conceptualization of governance. Consider his use of roll call "scales." They tell us something about the ordering of responses to parliamentary propositions but little about the generic nature of policy or policy making. Consider the instrument of political parties. How can we assess their comparative impact on policy formulation without tests of alternative influences? Statistics textbooks warn us to be on guard for "spurious" correlations. Consider the Senate in light of Brady's argument that policy realignments work through a turnover of congressional personnel. The upper house is structurally insulated from the direct effect of electoral change.

The contention that macroscopic policy shifts are explicable in partisan terms and that the proximate dynamic behind epic redirections in statecraft lies in popular electoral behavior is open to question. Realignment "theory," in other words, probably obscures as much about the governmental past as it illuminates. Arguably, the factors determinant of incrementalism, a disease that has incapacitated Congress in Brady's opinion, have had greater effect on shaping public policy than have critical elections. We need a different research design to weigh this possibility.

BALLARD C. CAMPBELL
Northeastern University

GILBERT C. DIN. *The Canary Islanders of Louisiana*. Baton Rouge: Louisiana State University Press. 1988. Pp. xiii, 256. \$32.50.

Confronted with the difficult task of defending Louisiana's extensive and vulnerable eastern boundary, the Mississippi River, with inadequate resources, the Spanish colonial administration sought to erect a viable barrier to anticipated Anglo-American incursions through the establishment of immigrant settlements at strategic points in the lower Mississippi Valley. Upon settlement, these settlers were immediately organized into militia units that constituted the colony's first line of defense against intercolonial aggression. In the late 1770s, after Acadian immigration had faltered, Louisiana actively recruited immigrants from the Canary Islands. Approximately two thousand Canary Islanders answered the call, boarding at least five Louisiana-bound vessels in 1778 and 1779.

Those Canary Islanders, or *Islenos* as they have come to be known in Louisiana, were established along the Amite River at Galveztown; along Bayou Lafourche at Valenzuela; on the northeastern shore of Lake Barataria, near New Orleans; and along Bayou Terre-aux-Boeufs in present-day St. Bernard Parish. Although

the Galveztown and Barataria settlements ultimately failed, the Valenzuela and Terre-aux-Boeufs settlements survived, and the distinctive subregional cultures of St. Bernard and Assumption parishes still bear testimony to the Hispanic contributions of the *Islenos*.

Although significant, these contributions have long been ignored by Louisiana historians who have generally tended to focus their attention on the Acadians or their French-speaking neighbors, the Creoles. Gilbert C. Din's study affords the first comprehensive examination of the Canarian migration to Louisiana and the inevitable transformation of the immigrants' transplanted culture in their new homeland. Din's work also examines the external and internal political, cultural, topographical, and economic forces that have shaped the course of the group's continuing evolution between the dawn of the nineteenth century and the modern (post-World War II) era.

The author, who maintains an admirable detachment from his subject, presents his findings in a highly readable, expository style. Din relates a fascinating story of human adaptation to environmental factors and cultural pluralism in southern Louisiana. But the narrative's chronological arrangement imposes certain limitations on the author, forcing him to deviate from his established chapter format whenever the volume of information proves too great to compress into a manageable size. In the first third of the book, individual chapters consistently examine only one of the various *Isleno* settlements. In the middle portion, they provide a survey of all of the surviving *Isleno* communities, despite the fact that these disparate communities were quite distinct and evolved in different directions. Near the end of the book, however, the author again reverts to his initial format but only in discussing the period *entre-les-guerres*. These organizational problems might well have been averted had much of the biographical data incorporated into the later chapters been either deleted or consigned to the appendixes.

This caveat aside, Din's study is an important work that fills a significant void in Louisiana and Gulf Coast historiography. Din has laid another stone in the foundation on which comparative, intercultural, and comprehensive transdisciplinary histories of the early Mississippi Valley can now be built.

CARL A. BRASSEAU
Center for Louisiana Studies
University of Southwestern Louisiana

ANN L. BUTTENWIESER. *Manhattan Water-Bound: Planning and Developing Manhattan's Waterfront from the Seventeenth Century to the Present*. New York: New York University Press. 1987. Pp. xxii, 243. \$35.00.

Navigation and commerce, recreation and traffic circulation, slum removal and sanitation—these concerns and more influenced the development of Manhattan's waterfront, as Ann L. Buttenwieser relates in this well-researched contribution to the historiography on

New York City. She shows how piecemeal reforms carried out by a variety of governmental bodies, from the municipal Dock Department to the U.S. Army Corps of Engineers, shaped Manhattan's shoreline in response to conflicting public and private needs and technological changes in ships and methods of pier construction.

Although her narrative extends to the present, Buttenwieser concentrates on the pre-1940 period. She explains how New York's policy of granting waterfront parcels to private individuals, with covenants requiring street and waterfront improvements, failed to create a shoreline that kept pace with the commercial needs of the eighteenth century, culminating in the creation of a municipal Dock Department in 1870. Yet, in trying to provide more adequate dock and storage facilities, the Dock Department encountered "a tradition of private development that the city would find hard to challenge," writes Buttenwieser (p. 75).

The construction of Riverside Park in the 1870s placed a green area close to the water's edge along the Upper West Side, although this recreation ground remained separated from the Hudson River by railroad tracks. In one of her most interesting chapters, the author explains how a variety of conflicting needs—from recreation to transportation to maintaining property values—came to bear on the problem of removing or covering these tracks. Not until the late 1930s, after four decades of controversy and thirty-six unimplemented plans, was this conflict settled through the autocratic planning of New York Park Commissioner Robert Moses. Research for this book was largely based on maps and files from the New York City Department of Ports and Terminals. Supporting the text are thirty-five interesting photographs, sixteen maps, and three tables.

To cover such a large expanse of history, the author has focused on waterfront developments in abstraction from other events occurring in New York City and the nation. And Buttenwieser, a city planner, does not explore analytical themes in her historical narrative, although she cites several themes in her introduction. The story of shoreline development is linked by superimposed subheadings rather than by analytical connections in the text. The result is a self-contained descriptive history that will be of interest mostly to those already familiar with New York City history.

RICHARD E. FOGLESONG
Rollins College

WILLIAM WYCKOFF. *The Developer's Frontier: The Making of the Western New York Landscape*. New Haven: Yale University Press. 1988. Pp. xiv, 239. \$27.50.

As its subtitle suggests, this study in historical geography aims at understanding how a portion of the American landscape came to be the way it is. More precisely, it includes a close examination of the lasting impact of the activities of a particular development

company, the Holland Land Company, on the western end of New York State between 1793 and 1812.

Although William Wyckoff chooses to treat his subject within a vaguely Turnerian framework, he takes pains to notice that the sequential stages of the master's archetype fail to fit the case at hand. And Wyckoff's "developer's frontier" does not work the same way as the "speculator's frontier" identified by Paul Wallace Gates and A. M. Sakolski or submit except in certain details to explanations based on concepts such as the "commercial" or the "urban" frontier. All of this Wyckoff makes clear in a brief historiographical discussion in his first chapter.

What distinguishes the "developer's frontier" as applied by Wyckoff to the "Holland Purchase," roughly the narrowing part of New York State lying west of the Genesee River, is the comprehensive role of the developer in planning, surveying, and promoting the settlement of an area previously uninhabited by whites; selling parcels of land at retail; investing in services such as mills and taverns; and building roads and otherwise encouraging commercial connections with trade routes outside the region. Wyckoff's detailed treatment of the Holland Purchase supplies a case study of a private development district, one of several such projects that accounted for a good share of the populating of western New York and the Ohio Valley during the years of the early republic.

Wyckoff's story is largely that of Joseph Ellicott, the surveyor and land agent to whom the absentee Dutch owners of the Holland Purchase entrusted the development of their three-million-acre tract. Relying heavily on Ellicott's correspondence and deed and survey records in Holland Land Company collections in various New York State repositories and one in Amsterdam, the author reconstructs in fine detail the successive stages of surveying, planning, establishing settlement and commercial policy, and actual settlement. Wyckoff's approach to his topic and the nature of his sources dictate that we learn much more about the role of the developer in the settlement of the area than about the settlers themselves.

The last two chapters deal with what appears ultimately to be Wyckoff's main interest, that is, the effect of Ellicott's policies on the landscape. One of those chapters provides a good lesson in the transforming effect of human ingenuity and activity on the land. The other contains a thoughtful discussion of the "legacy" of those transforming activities of nearly two centuries ago on the modern landscape, despite the subsequent building of the Erie Canal, the New York Central Railroad, and the modern city of Buffalo.

This book is a competent and intelligently organized case study in which a thorough use of manuscript sources is usefully supplemented by an inventive combination of maps and ground and aerial photography.

CHARLES E. CLARK
University of New Hampshire

ALF EVERS. *Woodstock: History of an American Town*. Woodstock, N.Y.: Overlook. 1987. Pp. viii, 749. \$37.50.

This volume was written primarily with a local audience in mind. Alf Evers, a thirty-year resident of Woodstock, begins by presenting a topographical view of the town from the top of Overlook Mountain and ends, almost seven hundred pages later, by reviewing the procession of people who have passed the village green. *Along the way he considers topics ranging from an analysis of the impact of various wars to the recounting of numerous legends*. He pays special attention to Woodstock's role as a tourist retreat, an artists' colony, and a haven for beats and hippies. Yet, as Evers indicates, the event most closely associated with the name Woodstock did not occur there. The three-day Woodstock festival of 1969 took place at White Lake in the town of Bethel some sixty miles away.

The book has many strengths. Written in a clear, conversational style and based on extensive research, it discusses a number of important issues. Residents of Woodstock might be surprised to learn that, although their town was home to Robert Livingston, one of the most ardent supporters of the American revolution, the bulk of the population consisted of Tories. Evers explains that seeming paradox by pointing to Livingston's huge land holdings and his repressive policies; his numerous tenants were certain to oppose what he supported. Also of interest is Evers's discussion of the Anti-Rent War of the 1840s, which involved another clash between the Livingstons and their tenants. In the course of the conflict, one Woodstock man was killed, another was tarred and feathered, and twenty-six were indicted on various charges. In the end, the state legislature abolished such abuses of the existing system as leases for three lives. The Livingstons soon had little influence.

The most interesting phase in Woodstock's history, however, began in 1902 when Ralph Radcliffe Whitehead selected a vast tract on a mountain as the site for his arts and crafts center. Whitehead, a wealthy Englishman, had studied at Oxford with John Ruskin. His goal was to create a community where human beings could express their creative potential while living in harmony with nature. He felt that such a community required a hierarchical structure. Many of the reformers and artists Whitehead attracted disagreed. Some of them moved off the mountain to take up quarters in Woodstock proper. Thus, an artistic population began to permeate the town as a whole.

Fascinating as Evers's treatment of all that is, professional historians will find the book wanting. Although Evers alludes to class distinctions within the community and bases much of his interpretation on those distinctions, he fails to present a systematic analysis of Woodstock's social structure at various stages of its growth. That is especially disappointing in a work on a community in New York for which state census manuscripts should be available as late as 1925. Evers's presentation of other topics seems superficial as well.

Moreover, as the title implies, the book merely tells the story of Woodstock. It lacks an overriding thesis. Evers does not use Woodstock to exemplify a particular type of community or the values of any specific group.

Still, Evers's work may have already had more of an impact on a general audience than most scholarly studies ever do. The *Woodstock Times* originally published it as a serial, and it is now available in a handsome, well-illustrated volume at a reasonable price. General readers will find it lively and informative.

CAROL A. O'CONNOR
Utah State University

JOHN H. HANN. *Apalachee: The Land between the Rivers*. (Ripley P. Bullen Monographs in Anthropology and History, number 7.) Gainesville: University of Florida Press and Florida State Museum. 1988. Pp. xiii, 450. \$36.00.

Early chroniclers described the Florida geographic area now called the Tallahassee hills as the famed "Province of Apalachee," whose inhabitants were depicted as "cruel and warlike" in one account and as "the best Indians in Florida" in another (p. 7). Drawing on Spanish, French, and English sources, as well as on recent archaeological and ethnohistorical research, John H. Hann has re-created this region in imaginative detail. He portrays Apalachee's peoples before the Spanish *entrada* (c. 1200–1500), during the expansion of the missions (1528–1675), and after the collapse of Apalachee society under pressure from both internal stress and external assault by English invaders (1685–1764).

In many ways this volume is both documentary source book and historical narrative. Readers will benefit from the translation of the Ball Game manuscript recorded by Juan de Pavia in 1676, an account that informs the reader about a fundamental sociocultural phenomenon practiced both in pre-Columbian times and, in modified form, by some tribes well into the nineteenth century. The appendixes include much-appreciated maps as well as detailed tables reflecting not only Apalachee town survival but also information about neighboring peoples. Of particular assistance is the glossary, which is actually an Apalachee handbook. This topic's inherent research difficulties are well illustrated by printing the documents describing Colonel James Moore's raids into Spanish Florida in search of slaves and conquest.

The author's skillful integration of ethnohistorical and archaeological data is exemplified by his presentation about the council house of the Apalachee. That large, round, wooden structure with a straw roof could contain fourteen hundred persons. It is reconstructed based on both historical documentation and site excavation reports from 1985 and 1986. Students of southeastern cultures who are familiar with Creek and Cherokee buildings described in William Bartram's

Travels and Henry Timberlake's *Memoirs* will find likenesses among the three. Such evidence of archaeological comparison and implied cultural connection reinforces many recent interpretations of the American Indian past in the Southeast. Hann's volume provides a model for those who will write the next generation of monographs.

Although some readers may be unhappy with Hann's practice of citing sources parenthetically within the text, that hardly detracts from his accomplishments. Others may tire of the author's tedious adherence to original documents, but the book's overall achievement is worth a close reading. Hann's creative study is an excellent companion volume to the first publication in this series, which was entitled *Tacachale: Essays on the Indians of Florida and Southeastern Georgia during the Historic Period* (1978), edited by Jerald Milanich and Samuel Proctor.

Just as Hann acknowledges his indebtedness to the pioneering borderlands investigations of Herbert Eugene Bolton and the seminal work on missions in Spanish Florida by Mark Boyd, we likewise will remain in Hann's debt for productively joining history, anthropology, and archaeology. Thanks to him, we know something about the Apalachee and their culture, even though they disappeared as a people more than two hundred years ago.

JAMES H. O'DONNELL III
Marietta College

BENJAMIN W. GRIFFITH, JR. *McIntosh and Weatherford, Creek Indian Leaders*. Tuscaloosa: University of Alabama Press. 1988. Pp. xiii, 322. \$26.95.

William McIntosh and William Weatherford are good candidates for a dual biography. Born during the American revolution to Creek mothers and white fathers, both moved easily back and forth across the frontier between cultures: they spoke English as well as Creek and became prominent planters as well as important warriors. Added to these similarities are intriguing differences. In the Creek War of 1813–14, Weatherford joined the Red Sticks' nativist uprising; McIntosh fought alongside Andrew Jackson's troops. After the war Weatherford returned to private life and died, a respected figure, in 1824; McIntosh, tying himself more closely to the United States, embarked on a public career of fraud and chicanery that ended in 1825 when his fellow Creeks executed him for illegally signing away Creek lands. In this book Benjamin W. Griffith, Jr., uses these two men "to focus on the Creeks in writing a history of Indian-white relations in the Creek Nation" (p. xi).

Griffith fails to do the topic justice. Instead of offering a window onto a Creek world in the midst of rapid change—a traditional society coping with such novelties as cotton and cattle, black slaves and Christian missionaries—the author confines himself to accounts of treaties and battles told largely from the white point

of view. Worse still, Griffith adopts not only the perspective but also the biases of his white sources. Thus, he brands the nativist Red Sticks as "revolutionaries," though the real revolutionaries were those backing the federal government's campaign to destroy Creek culture and acquire Creek lands (p. 88). He labels those nativists "fanatical," forgetting that they were no more committed to their cause than Jackson was to his (p. 126). He tends to call Red Stick victories "massacres" and the victors "murderers" (even when they lost half of their men in an action), while generally terming Jackson's triumphs "battles" and the winners "gloriously victorious," even when whites lost only five killed to their foes' 186 and some of those 186 were women and children (pp. 99, 126, 119, 149). And Griffith considers Indians who opposed Jackson "savages," their white conquerors "heroic" (pp. 189, 148).

The book's treatment of McIntosh also suffers from bias. The author presents extensive evidence of McIntosh's deceit and bribery yet in the end tries to defend the man, presumably because he so consistently sided with whites. Speculating that McIntosh, who received forty thousand dollars for signing that illegal treaty, "believed sincerely" that the Creeks would be better off elsewhere, Griffith concludes by calling him "entrepreneurial," a "pragmatist" whose mistake was that he "badly miscalculated the ingrained intransigence of [his] nativistic" opponents (pp. 269, 272).

Griffith's interpretation of Indian-white relations recalls an earlier day, when scholars adopted the white point of view with no pretense to giving the Indian side of the story. In this volume the pretense is there, but the point of view remains what it was a generation (or even a century) ago. Weatherford, McIntosh, and the Creek Nation deserve better.

JAMES H. MERRELL
Vassar College

RICHARD G. BREMER. *Indian Agent and Wilderness Scholar: The Life of Henry Rowe Schoolcraft*. (A Clarke Historical Library Sesquicentennial Publication.) Mount Pleasant: Clarke Historical Library, Central Michigan University. 1987. Pp. x, 445. \$30.00.

On October 12, 1823, United States Indian agent Henry Rowe Schoolcraft married Jane Johnston at Sault Sainte Marie, Michigan. The mixed-blood daughter of a prominent local trader, she gave Schoolcraft access to her Indian relatives who revealed details of Chippewa culture unknown to outsiders. Years of study and writing and self-promotion ultimately secured Schoolcraft a reputation as America's leading Indian scholar who helped shape the public image of American Indians.

The focus of this volume is Schoolcraft's career as a field representative of the Office of Indian Affairs and as an ethnologist, yet the author outlines Schoolcraft's other occupational interests. Prior to employment with the Office of Indian Affairs, he was a talented glass-

maker, an analytical chemist, a mineralogist, geologist, explorer, and travel writer. In 1822, Congress established an Indian agency at Sault Sainte Marie. Schoolcraft's political connections landed him the job as agent. For the next nineteen years he served in that capacity and as Michigan superintendent of Indian affairs. Thereafter, until his death in 1864, he earned a living as an authority on Indian cultures: lecturing, conducting a census of Indian tribes for New York as well as for the federal government, and publishing widely in popular literary magazines and government-sponsored volumes.

Richard G. Bremer is at his best when analyzing four facets of this remarkable nineteenth-century man. Evaluation of Schoolcraft as an Indian Office employee demonstrates Bremer's grasp of the transitional course of Great Lakes Indian relations during Schoolcraft's tenure. The many duties of the agent are also scrutinized, from accounting practices to treaty negotiations. All in all, the author judges that "in an essentially untenable situation he [Schoolcraft] did what he could for a cause [protection of his Indian charges] doomed by the realities of power and the cultural assumptions of the day" (p. 214). Bremer is somewhat less charitable about Schoolcraft the ethnologist. His prolific publications are scrupulously analyzed, with the conclusion that they were poorly organized and lacked both originality and intellectual depth. But Schoolcraft was influential. In addition to shaping public thinking about Indians, he gathered from many sources and presented to his audiences a rich variety of ethnological materials. Bremer's psychological assessment of his subject's personality is believable and hence helpful in explaining how Schoolcraft's "deep rooted and pervasive sense of insecurity" (p. 348) triggered a lifelong quest for fame and approval. A final facet of this probing study sets Schoolcraft in the context of his times and reveals the author's superb analytic powers and historical understanding.

For these and other reasons, Bremer's illuminating biography makes a major contribution to Great Lakes as well as American Indian studies. No other examination of Schoolcraft is so carefully researched, so thoughtfully and objectively written, nor does any other study explore so thoroughly his life as an Indian agent and ethnologist. True, Schoolcraft was not a great American. But neither should his considerable contributions be neglected. They will not be any longer.

EDMUND J. DANZIGER, JR.
Bowling Green State University

MARC EGNAL. *A Mighty Empire: The Origins of the American Revolution*. Ithaca: Cornell University Press. 1988. Pp. xv, 381. \$36.95.

In yet another effort to push the later American penchant for a two-party political system deep into the prenational period, Marc Egnal seeks through the

experiences of five colonies—Massachusetts, New York, Pennsylvania, Virginia, and South Carolina—to reinterpret both the nature of political contention in colonial British North America and the origins of the American revolution in terms of what the author perceives as a fundamental conflict between two opposing “persuasions” (p. 9), which he calls “expansionism” and “nonexpansionism.” Although he explores in depth neither the content nor the ideological dimensions of these “*broad, ramifying world view[s]*,” he describes expansionism as a commitment “to promoting the ascendancy of America” by strengthening “local sovereignty,” maintaining a healthy domestic economy and a vigorous maritime commerce, and facilitating the rapid development of new lands. By contrast, nonexpansionism, “while not opposed to growth,” was characterized by a manifest “lack of confidence in the present strength and future prospects of the New World” (p. 7). According to the author, these antagonistic outlooks formed the ideological basis for the development of “two elite parties” (p. 17) that in each of the colonies he considers—and, he suggests, probably also in the other eight—structured political debate and conflict throughout most of the eighteenth century.

This tidy reading of eighteenth-century American political history consists largely of a series of sequential—and highly selective—sketches of the political history of the five model colonies, constructed to illustrate the salience of the division between expansionism and nonexpansionism. Whereas before 1763 adherents of those opposing persuasions battled over colonial autonomy, monetary policy, and military expenditures, between 1764 and 1776 they contended over how far to go in opposing British colonial policy. “In every colony,” the author concludes, expansionists, notwithstanding challenges from below by farmers and artisans mobilized during prerevolutionary protests, “not only supported the struggle against Britain but . . . later plump[ed] for the Constitution as an important step in the rise of the United States” (p. 7). Nonexpansionists, in contrast, opposed both vigorous opposition to Britain and the Constitution.

Having learned from recent studies of individual colonies that the political experience of each of those largely discrete and widely disparate entities was highly distinctive, early American historians will be skeptical of an interpretation that finds so few variations over time and space and will be suspicious that the author’s categories have been imposed on, rather than derived from, the data. Those feelings will be nourished by several obvious problems. First, the evidentiary base needed to establish the author’s central proposition is either undeveloped or lacking. Despite a long appendix listing “prominent party adherents in each province” (p. xi), the volume shows little indication that the author has done the detailed prosopographical work required to establish credible profiles of political behavior and attitudes for the vast population of legislators whose behavior he purports to explain. In New York,

the only one of the five colonies for which concrete evidence on political divisions exists in the form of abundant legislative roll calls, the author has not performed the types of statistical analyses conventionally used to establish whether or not consistent voting blocs existed and, if so, how important the issue of expansion as he defines it might have been in relation to other variables in determining how those blocs voted. Although factions were operative at various times in Massachusetts and Pennsylvania, surviving evidence provides no firm basis for testing the author’s characterization of those factions for either province. In Virginia and South Carolina, evidence for party divisions of any kind is so fragmentary and inconclusive that the author’s factions are almost certainly fictions.

Even though those evidentiary problems prevent the author from proving his thesis for any, much less for all, of the colonies and even though he admits that “colonial factions were rarely rigidly defined” (p. 339), he proceeds to make those factions the active agents in his political narratives and thereby reifies and endows them with a degree of coherence and continuity and a defining power substantially greater than any contemporary evidence would seem to warrant. Similarly, although the author takes pains to denounce “the simplicity that comes from a deterministic approach to history” (p. 10) and acknowledges that political divisions also “reflected the influence of self-interest, religion, and national origin” (p. xi), he explicitly characterizes such influences as mere “crosscurrents” in comparison “with the Gulf Stream of ideological outlook” (p. 339) represented by the all-encompassing conflict between expansionism and nonexpansionism. A few readers may find such a reductionist and monocausal explanation attractive. For most members of a generation schooled in a growing appreciation of the rich intricacies and ambiguities of human existence, however, it will seem egregiously formulaic, simplistic, and distorting.

Although the author has adduced enough evidence to add credence to suggestions by several earlier historians that at some deep level key political actors disagreed about the capacity of America to function as an independent entity and that those disagreements had some influence on the politics of colonial and revolutionary America, he has not demonstrated that those disagreements provide the key to understanding early American politics and the American revolution. Their precise role remains to be explained. By inadvertently calling attention to the need for such explanation, this volume has contributed to the ongoing search for understanding that complex event known as the American revolution.

JACK P. GREENE
Johns Hopkins University

DON R. GERLACH. *Proud Patriot: Philip Schuyler and the War of Independence, 1775–1783*. (A New York State

Study.) Syracuse, N.Y.: Syracuse University Press. 1987. Pp. xiv, 636. \$45.00.

This book is the second of Don R. Gerlach's projected three-volume biography of Philip Schuyler. In the present volume, Gerlach follows Schuyler's military and political career during the American revolution, with a heavy emphasis on the years 1775–78, during which Schuyler commanded the Northern Department, was relieved from that command, and eventually exonerated by a military court. Thus, the author devotes eleven of the book's sixteen chapters to this period, while the remaining five chapters deal with Schuyler's political activities in both New York State and the Continental Congress as well as his participation as a member of the Northern Board of Indian Commissioners.

Gerlach's avowed mission in undertaking this ambitious study is to rescue Schuyler "from the neglect to which time and circumstances have consigned him" (p. xii). Moreover, the author attempts to rebut those historians who have viewed Schuyler as a vain, quarrelsome, and less than competent commander of the Northern Department. Of course Schuyler never viewed himself in this light, and since Gerlach informs us that "in order to understand a man one must be his friend" (p. xiii), it is not the author's view either. Indeed, so closely does Gerlach identify with his subject that the book often seems more autobiographical than biographical. The author accepts uncritically Schuyler's account of events and his vilification of his critics and rivals. I grant that a biographer must view his or her subject with understanding and sympathy, but he or she should do so with a greater degree of critical detachment than one finds in this volume.

Unquestionably Schuyler was often treated unkindly, and perhaps unfairly, by his contemporaries, and historians have generally accepted those judgments concerning his character and abilities. A more objective assessment of Schuyler reveals that his performance as the commander of the Northern Department was at least equal to that of his principal rival and successor, Horatio Gates, and Schuyler's administrative abilities were markedly superior. Although the military reverses suffered by the Northern Army while under Schuyler's command cannot be ignored, neither can they be simply attributed to a lack of judgment and leadership on his part. Serious logistical and manpower deficiencies, combined with the incompetence and insubordination of Schuyler's subordinates can be blamed for most of the defeats suffered by the American army in Canada and northern New York. Nevertheless, historians must recognize and explain the fact that many of Schuyler's contemporaries believed that he was responsible for those failures and worked diligently to have him replaced with a more vigorous general. Eventually, Schuyler's congressional critics chose Gates to play that role, initiating the Schuyler-Gates controversy, which impaired the operations of

the Northern Army and seriously divided the Continental Congress in 1776 and 1777.

In retrospect it is clear that the attacks on Schuyler that eventually led to his removal were grounded on more than base scheming by his political enemies or Gates's ambition, as Schuyler would have had, and Gerlach would have, us believe. Instead, fundamental ideological differences and regional antipathies (particularly between New York and New England) fueled the Schuyler-Gates controversy in and out of Congress. The author makes little or no attempt to explain how and why these factors played a major role in Schuyler's problems with both the men he commanded and his congressional critics. By consistently acting as Schuyler's advocate before the court of history, Gerlach simply defends Schuyler's perception of his problems and thus leaves the reader with an incomplete understanding of the larger issues involved.

Although this biography is not without flaws, Gerlach has accomplished what he set out to do, rescuing Schuyler from the obscurity to which he had been relegated, restoring his reputation, and securing for him his rightful place in the pantheon of the revolutionary era. The author's meticulous scholarship, extensive use of contemporary sources, and lucid narration produce a richly textured portrait of a complex and talented man.

JONATHAN G. ROSSIE
St. Lawrence University

REGINALD C. STUART. *United States Expansionism and British North America, 1775–1871*. Chapel Hill: University of North Carolina Press. 1988. Pp. xvi, 374. \$37.95.

"This book," Reginald C. Stuart states in his preface, "explores the cultural dynamics behind America's external relations with . . . British North Americans on the northern frontier of the United States" (p. xii). Formal diplomatic relations scarcely interest him and are only cursorily treated. Stuart not incorrectly discerns American expansionism at work between 1783 and 1871, but he emphasizes that it "was a broad phenomenon with cultural, demographic, diplomatic, economic, ideological, political, and territorial ramifications" (p. 257). He finds little to criticize in the process and indeed stresses those portions of it that, apparently paradoxically, eased relations with British North America and with Britain.

Two themes are woven in and out of Stuart's study. The first is the creation and growth of a "borderland," an area spanning national boundaries that developed economic, demographic, and cultural ties. In that area, especially after the War of 1812, Americans gained a predominant role. Unlike many of their fellow U.S. citizens, who at least in early years viewed the British provinces as a menace, Americans in the borderland saw British North Americans as "neighbors, markets, relatives, and friends" (p. 53). Their expansionism, then, was benign rather than bellicose.

Stuart's second theme—popular attitudes, in the borderland and elsewhere, toward the possible acquisition of British North America by the United States—has two distinct parts. We should not misunderstand agitation for conquest during the American revolution and again at the time of the War of 1812, he says, since “what seemed like territorial expansionism actually arose from a defensive mentality, not from ambitions for conquest and annexation” (p. 76). The primary aim was not to conquer British North America per se but either to eliminate threatening bases or merely to force Britain into concessions on other matters. Stuart's argument may be correct—most of those who have written recently on the War of 1812 would agree—but a great deal of imperial expansion has been justified in terms of defense, and it is at least permissible to wonder whether, had the expansionists succeeded in 1776 or 1812, the United States would have abandoned captured territory.

Following the War of 1812, “defensive expansionism” (p. 60) was displaced by what Stuart calls the “convergence thesis” (p. xi). For French Canadians, denigrated as slothful and Catholic, Americans had little hope, but they did expect Canadians of English descent, though somewhat inferior as the result of life under a monarchy, to be Americanized as a consequence of increasing contact, particularly trade. Many Americans were convinced that there was “a causal relationship between economic exchange and political union” (p. 235) and expected the peoples to the north to join the United States; others foresaw an independent Canada, separated from Britain but economically tied to the United States.

Aside from a small rabble for whom Stuart has scant sympathy, almost all Americans believed that the ultimate decision about political union must be made by the Canadians themselves. “True to their own ideology,” he writes, “Americans considered mutual consent a vital prerequisite to union with the provinces” (p. 96). (The Mexicans would have welcomed a similar concern by Americans for ideology.) But many hoped and even expected that economic “convergence,” with a consequent Americanization of Canadian values, would ultimately produce political union.

As the idea of conquest, even defensive conquest, receded, softer political attitudes logically followed. The Webster-Ashburton Treaty and the Oregon settlement meant, Stuart says, that by 1846 “Anglo-American detente about sharing North America had emerged, whatever the rhetorical excesses of Manifest Destiny extremists” (p. 105). Perhaps so, but how is “sharing” to be reconciled with “convergence”? Finally, Stuart argues, the Treaty of Washington in 1871 “represented a symbolic recognition of Confederate Canada's semiautonomy” (p. 252). For reasons Stuart barely probes, the dream of political convergence had joined defensive expansionism in the dustbin of history.

Inevitably, given its chronological sweep, this book is not based on intensive research in manuscript materi-

als. Stuart has examined some archives—administrative and consular records—of the U.S. government, and he has also used the private papers of a small number of men. In printed materials, however, his research has been enormous; even though some items cited in the massive footnotes are not listed in the bibliography, the latter extends to thirty-six pages, ranging from historical monographs to antiquarian descriptions to collected correspondence, periodicals, and pamphlets.

Few would quarrel with Stuart's depiction of the growth of a borderland, and, indeed, although no one has developed that theme as comprehensively as he, most of its components are familiar. Much the same could be said of his examination of defensive expansion. His argument for the convergence thesis is the most original—and longest—part of the book. On the whole, the argument is convincing, if sometimes forced. Less convincing is the corollary argument that the rise of the convergence thesis between 1815 and 1860 helped to produce “a strengthening of Anglo-American diplomatic accord” (p. 79). Surely contemporaries would not have seen the events of those years in that light.

BRADFORD PERKINS
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DAVID F. LONG. *Gold Braid and Foreign Relations: Diplomatic Activities of U.S. Naval Officers, 1798–1883*. Annapolis, Md.: Naval Institute Press. 1988. Pp. xvii, 502. \$29.95.

In 1912, Johns Hopkins University Press published Charles O. Paullin's *Diplomatic Negotiations of American Naval Officers, 1778–1883*, a volume in the Albert Shaw Lectures on Diplomatic History. Now, more than seventy-five years later, David F. Long has undertaken to revise, update, and expand Paullin's study. The result is a monumental and comprehensive resource that chronicles all activities of U.S. naval officers who influenced U.S. foreign relations from 1798 to 1883. It will surely serve as an important point of departure for historians interested in the nexus between diplomatic and military history in the years from the early establishment of an independent Navy Department to the naval renaissance of the late nineteenth century.

Long considers Paullin's work inadequate because it “ignores the disparity in potency and to some extent in aims between the pre- and post-Civil War navy” and because it is limited to the “diplomatic negotiations” of naval officers (p. xiv). Long attempts to correct these and other deficiencies. He distinguishes eight categories of activities of U.S. naval officers: protection and enhancement of commerce; war making; peacetime aggression; treaty making; non-belligerent diplomacy; affiliations with State Department employees; humanitarianism; and expansionism. Through both a chronological and a geographical approach, Long analyzes the

impact naval officers had on U.S. foreign relations, either as direct agents of the president or, more important, as decision makers themselves, assuming authority in the absence of timely communication with government officials. Overlapping responsibilities with State Department officials complicated matters (who actually commanded whom was an issue of continuing controversy), and, with so many duties to perform besides keeping their ships in fighting trim, U.S. naval officers often found themselves in diplomatically precarious situations. In the years before the communications revolution, months might elapse before messages passed to and from Washington. Thus, the government granted wide latitude to an officer's intelligence and common sense. Sometimes patience, tolerance, tact, and perseverance ruled the day. All too often, arrogance, indiscretion, force, or the threat of it, did.

Long describes and categorizes an enormous array of events and personalities from well-known stories about Charles Wilkes, David Porter, and Thomas ap Catesby Jones to America's first ultimatum, first annexation of noncontiguous territory, and first interferences in the domestic affairs of other nations. He also includes the most innocuous, obscure, even accidental actions that can possibly be defined as influencing foreign relations. Long has consulted most of the important English-language primary sources and secondary works. Undoubtedly his book will enhance future studies of early American diplomatic and military history.

Long contends that his analysis of over five hundred incidents shows the "close relationship between American economic, political, and diplomatic trends, on one hand, and the contribution of U.S. naval officers to their country's foreign relations, on the other" (p. 416). Although hardly anyone will find this conclusion surprising or disturbing, neither will they find it particularly novel. What some readers might find more provocative will be the evaluation that most U.S. naval officers' diplomatic activities can be rated as successful. Long details so many American breaches of neutrality, so much arrogance, ineptitude, incompetence, and stupidity by some U.S. naval officers that it is difficult to reconcile such unimpressive behavior with an assessment, statistically arrived at, that naval officers successfully promoted U.S. foreign policy objectives. In these years it seems that luck rather than design ultimately secured U.S. aims. There are other questions that arise from too brief an analysis of U.S. foreign policy goals and too superficial a discussion of international law. Some readers will find Long's repetition and narrative detail out of balance with his occasionally preemptive conclusions. Still, Long claims his object has been to provide a reference work, and in that regard he has produced one of the most meticulously documented, valuable resources ever written for diplomatic or military historians.

JOYCE S. GOLDBERG
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Arlington

JEFFERY A. SMITH. *Printers and Press Freedom: The Ideology of Early American Journalism*. New York: Oxford University Press. 1988. Pp. ix, 233. \$24.95.

During the past twenty-five years, the standard work on the origins and meaning of the First Amendment has been Leonard Levy's *Legacy of Suppression* (1960). In it Levy argued that the framers of the U.S. Constitution had a very narrow view of the meaning of freedom of the press, especially regarding political questions. More specifically, Levy claimed, the original intent of the First Amendment was to provide freedom from any kind of censorship prior to publication, but after publication an author and printer could be held accountable and prosecuted for seditious libel, and until 1798 not even the truth of a claim was always a sufficient defense.

Levy's interpretation has been challenged. A number of scholars have pointed out that, regardless of what theoretical limits may have existed on the freedom of the press, newspapers in the last half of the eighteenth century were pretty much able to publish what they wanted with impunity. Responding to this criticism, Levy published *Emergence of a Free Press* in 1985, a substantially revised version of his earlier work. In it he admitted that in practice the early American press was a good deal freer than he had recognized, but he stood by his claim that the First Amendment was intended only to protect the press against prior restraint.

Jeffery A. Smith believes that Levy did not take his recantation far enough. He argues not only that the press in early America was actually free but also that a libertarian ideology existed that justified and defended the press and that the meaning of the First Amendment was much broader than Levy has claimed. Smith begins by surveying the English and European intellectual background of the idea of a free press. He also examines a number of the political and legal questions raised by the debate over Levy's work. Finally, he takes a close look at the ideas and practices of Benjamin Franklin and several other printers who at various times were associated with him. Along the way Smith makes some interesting points. Of particular significance is his claim that many Americans believed that the press should be free from all restrictions by the government but should continue to be liable for attacks on individuals.

Unfortunately, the book has problems, and those outweigh its virtues. To begin with, this is not so much a detailed study of a complicated issue as a sweeping and selective survey. Much of the book covers material that is well known, and the work is descriptive rather than analytical. What is really new and original could have been presented in a more effective and focused manner in an article or two. Also the treatment of the ideological background of a free press tends to be superficial and vague, especially in regard to the discussion of Enlightenment values. And, although Smith places considerable emphasis on the importance of radical Whig ideas, he seems oblivious to the fact that

there is now considerable debate among scholars about how important and pervasive those ideas really were. Finally, Smith tends to overstate his argument and the significance of his evidence. Printers and radical agitators may at times have argued that the press needed absolute freedom to prevent the government from abusing its powers, but he never demonstrates that the belief was shared by the actual framers of the First Amendment. Nor does he make any attempt to understand the thinking of people such as George Washington, James Wilson, Alexander Hamilton, and John Adams, all of whom favored prosecution for seditious libel and did not accept the distinction between attacks on the government and the defamation of individuals. He also does not adequately explore the Jeffersonian position. He only looks at the Jeffersonians' opposition to the Sedition Act of 1798 and does not deal with the belief held by Thomas Jefferson himself that the First Amendment prohibited prosecutions by the federal government but not by state courts.

In sum, Smith makes a small contribution to the rapidly intensifying debate over the nature of freedom of the press in the early republic, but his book does not supersede Levy's intelligent, knowledgeable, incisive and provocative, if controversial and flawed, treatment.

RICHARD E. ELLIS
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DAVID S. REYNOLDS. *Beneath the American Renaissance: The Subversive Imagination in the Age of Emerson and Melville*. New York: Alfred A. Knopf. 1988. Pp. x, 625. \$35.00.

The title of this book might lead us to expect a reprise or rebuttal of F. O. Matthiessen's classic *American Renaissance: Art and Expression in the Age of Emerson and Whitman* (1941). David S. Reynolds does indeed deal with the same cast of major characters, while adding Edgar Allan Poe and Emily Dickinson to the pantheon. Unlike many of today's literary scholars, he does not directly challenge that canon. The greatness of Ralph Waldo Emerson, Henry David Thoreau, Nathaniel Hawthorne, Walt Whitman, and the rest is not in doubt for him. What Reynolds challenges in a book of remarkable verve, comparable in length and richness of allusion to Matthiessen's work but otherwise very different, is the view that the foremost figures of the nation's literary past were isolated and estranged from the American mainstream.

This argument does lead Reynolds to contend that, if Emerson and company deserve to be treated as major authors, they were not the only writers of their particular stripe. Often, he says, their work differed in degree rather than in kind from that of a number of unduly neglected authors, women in particular. He is mainly concerned, however, with another form of scholarly neglect, that of a racy, slangy, sensational, sometimes erotically "subversive" American word cul-

ture well known to the canonical *littérateurs*, and generally relished by them, though slighted or ignored by twentieth-century academicians.

One of Reynolds's favorite exemplars of this subculture is the Philadelphian George Lippard (1822–54), a selection of whose hectically radical extravaganzas Reynolds has recently edited. Lippard, widely read in his day, was in truth almost forgotten until the revival of his fantastic *Quaker City, or The Monks of Monk Hall*. He admired and befriended Poe and founded the mysterious reformist Brotherhood of the Union. Lippard is a fascinating figure to set beside more obviously lurid scribblers such as George Thompson or folk preachers such as Edward Taylor, the probable model for Father Mapple in Melville's *Moby-Dick*. Reynolds offers fresh insights into the deliberate paradoxes of democratic republicanism: "immoral" or "dark" reform, subversive rhetoric as pioneered by John Neal, and so on. Although his readings are most convincing when he deals with fiction (including *Uncle Tom's Cabin*), Reynolds has excellent things to say about all of his chief writers, Dickinson among them. He has identified and illuminated genuinely "subversive" yet not taboo aspects of the American Renaissance: calculated hysterias, didactic melodramas, verbal excesses designed to portray and rebuke the actual excesses of alcohol, sex, homicide, and less stark immiseration.

Not every part of this volume overwhelms the skeptical reader. Occasional exceptionalist statements make one wish there had been room for fuller treatment of comparable British popular and sensationalist culture, as represented by G. M. W. Reynolds or the punster-humanitarian Tom Hood, not to mention Charles Dickens. Little is said about the theater and not much about western and southern vernacular or low-life styles. There is a slight tendency, detectable also in kindred historical interpretations of Jacksonian America by Sean Wilentz and others, to romanticize the virtues of street republicanism. Burlesques and other high-spirited protests are splendidly analyzed. What, though, of racist, nativist, sexist manifestations of popular culture? What of the ambiguities in Whitman or in Twain, who were never wholeheartedly sure, like Emerson, Thoreau, and Hawthorne, how entirely to commit themselves to identification with the common man? What of the best-selling or at any rate prominent male authors of the era who constituted its canon? John Greenleaf Whittier is not in the index. Henry Wadsworth Longfellow figures mainly as an upholder of genteel orthodoxy. Yet, arguably, mainstream values were predominantly genteel, consigning the modes that Reynolds emphasizes to sometimes covert or embarrassed usage.

Yet, even if he has overstated his case, in the interests of bold advocacy, he has still done an excellent job. Attention does deserve to be paid to textures of American life that have not had their due. Social historians will gain intriguing new material from this book. If they begin, as a result, to allot more space to literary sources at all levels, such an outcome is most welcome.

Reynolds is to be congratulated on a study that, although it does not exactly supersede Matthiessen, merits a place on the same special shelf.

MARCUS CUNLIFFE
George Washington University

WILLIAM H. GERDTS AND MARK THISTLETHWAITE. *Grand Illusions: History Painting in America*. (The Anne Burnett Tandy Lectures in American Civilization, number 8.) Fort Worth: Amon Carter Museum; distributed by University of Texas Press, Austin. 1988. Pp. 174. \$21.95.

This book of three lecture-essays is not a comprehensive survey of American history painting as its title implies. Mark Thistlethwaite and William H. Gerdts focus on the middle decades of the nineteenth century, when history painting was reputedly in decline. The authors argue, instead, that this art blossomed in the 1840s and 1850s.

Yet, in Thistlethwaite's opening essay, evidence for the vitality of mid-nineteenth-century history painting remains problematic. Thistlethwaite demonstrates that American history painting oscillated between idealism and realism but concludes that the evolution was in the direction of realism. Thus, under the impact of democratic change, the academic grand manner disintegrated into hybrid categories like "genreified history" and "historical genre," which celebrated ordinary daily activities and the common man (p. 44). Furthermore, when Thistlethwaite examines the period's most famous example of the grand manner, Emanuel Leutze's *Washington Crossing the Delaware* (1851), he states that it is "grounded in materialism" and lacks the idealism of traditional history painting (p. 45).

In the second essay, Gerdts insists that the proliferation of hybrid categories, including historical landscape and historical still life, proves the expansive power of history painting. One could argue with more legitimacy, however, that the hierarchy of subject matter was collapsing and that the civic republican ethos of history painting was diluted, not strengthened, by the rise of landscape, genre, and still life painting.

Ignoring aesthetic quality as a signifier for vitality or conviction, the revisionist Gerdts stresses the quantity of production. He identifies numerous neglected artists and exhibited pictures, many now lost. Although he does not adequately evaluate any single artist or painting, Gerdts nevertheless appears driven to elevate the prestige and, perhaps, the marketability of this art through favorable association with the more glamorous European art world. He even asserts that "American history painting triumphed rather than failed, when compared with developments across the Atlantic" (p. 64).

Gerdts is not comparing quality here but rather the degree of critical praise and public acceptance. Beyond quoting amateur art critics who pledge fidelity to the academic dicta, however, Gerdts fails to address the

nature of the American audience that made the paintings a "triumph." That audience is an undifferentiated abstraction. Gerdts vaguely suggests that history painting flourished because "democratic institutions maintained their force" (p. 83), that is, the paintings appealed to the generic common man.

Unsurprisingly, therefore, in the book's final essay, devoted to the populist Düsseldorf School, Gerdts does not think to consider the flood of German and Irish immigrants entering America in the 1840s and 1850s. Do the revolutionary sentiments expressed by many Düsseldorf paintings speak only to a German context, or does their successful exhibition in the United States suggest that they raised ethnic, religious, and class issues in an increasingly heterogeneous, industrialized nation? Gerdts gives no clue, and his faceless audience makes his "triumph" of history painting ring hollow.

DAVID BJELAJAC
Tufts University

DAVID BJELAJAC. *Millennial Desire and the Apocalyptic Vision of Washington Allston*. (New Directions in American Art.) Washington, D.C.: Smithsonian Institution. 1988. Pp. xiv, 226. \$35.00.

David Bjelajac's book is an in-depth study of Washington Allston's unfinished painting *Belshazzar's Feast* (1817-43) within the context of American thought of the time and against the backdrop of the events unfolding in the painter's life. The painting was meant to be interpreted as much more than simply a picture of Daniel pointing out to the astonished Babylonian courtiers the disembodied finger writing on the wall of the palace. As Bjelajac explains, "New England intellectuals, from Cotton Mather, taught successive generations of Americans to think of their history in providential terms as a spiritual antitype for events described in the Old Testament" (p. 155). Significantly, the painting was conceived during the time of Allston's own religious reawakening in the aftermath of his first wife's death and during the defeat of Napoleon, whose antireligious excesses the painter had seen while in Paris. Allston stood diametrically opposed to Thomas Paine's *Age of Reason*, a best seller during the late 1790s, which ridiculed reliance on the precepts of the Old Testament. While *Belshazzar's Feast* was being painted, religious revivalism was sweeping America, and that revivalism augmented the identification of Belshazzar with Napoleon and of Daniel with George Washington or Daniel Webster (it is not clear from the book which one). The millennial aspirations of the young republic were seen in terms of the perceived perfection of the society of the ancient Hebrews, as clergymen identified Americans as God's new chosen people.

Much of the book's merit lies in Bjelajac's setting forth, through a liberal sprinkling of quotations from the sermons, speeches, and pamphlets of the time, the various points of view regarding the Bible, nationalism, and rationalism. During his stay at Harvard, Allston

confronted among his fellow students the "bucks," who favored extreme rationalism and were "men of fashion and splendor" (p. 59), utter libertines who on occasion disturbed peaceful citizens with their street brawls. Among the liberal clergymen, the Reverend William Ellery Channing, Allston's brother-in-law and friend, was preeminent. Although Channing opposed the more mystical aspects of Christian theology and denied that Christ's death atoned for the sins of humankind, he insisted that salvation could not be reached by reason alone and condemned the revolutionary principles promulgated by the French, as he praised Washington over Napoleon for "his sublime moral qualities" (p. 118). Then there was the English poet Samuel Taylor Coleridge, who insisted that the Old Testament prophets were the most reliable guides for solving current political problems, while Babylon's soothsayers could be compared with the advocates of French revolutionary ideals. By using Allston's painting as his focus, Bjelajac, in widening circles, conveys the flavor of the theological controversies of the age.

ABRAHAM A. DAVIDSON
Temple University

ANNE TAYLOR. *Visions of Harmony: A Study in Nineteenth-Century Millenarianism*. New York: Clarendon Press of Oxford University Press. 1987. Pp. 285. \$46.00.

This book is a speculative biography of two men, George Rapp and Robert Owen, who, in the 1820s, were the dominant figures on the utopian landscape. Necessity brought them together when Rapp sold his community at Harmony on the Wabash River to Owen, who was eager to root his vision of rational harmony in the New World. Much of the book covers well-worn territory developed by other biographers. What distinguishes this account is the interpretation that Anne Taylor weaves into the narrative.

Owen emerges as a financial schemer and zealot, while Rapp comes off slightly better as an autocrat and seer. This view of Rapp is consistent with Karl Arndt's perspective, and Taylor has drawn heavily on unpublished material about Rapp from Arndt's warehouse of documents. Her view of Owen is at odds with that of John F. C. Harrison, whose *Robert Owen and the Owenites in Britain and America* (1969) depicts Owen both as reformer and as ideological champion of emergent labor and entrepreneurial values. Harrison's interpretation is on firmer ground.

There is considerable history by innuendo in this volume. Taylor hints at Frances Trollope's "schoolgirl crush" on Frances Wright (p. 182) and Robert Owen's support for the use of contraceptive devices (p. 173). The first assertion rests on some shaky linguistic interpretations and the second on a highly speculative reading of a passage from George Jacob Holyoake's *Autobiography*. Trollope's "infatuation" is seen differently by Wright's latest and best biographer, Celia Morris Eckhardt, who focuses her analyses on the cause

of Trollope's American sojourn, which, according to Eckhardt, was rooted in Trollope's need to evade creditors rather than to bask in Wright's company. At one point Trollope did believe that Wright was the most interesting woman in Europe. That is not the same as a "crush."

Taylor has an easy and winning style and is able to give shape and life to her subject. She did that admirably in her earlier biography of Laurence Oliphant, the traveler, mystic, and reporter. Here, however, style has gotten the best of the book, whose subtitle suggests a study of nineteenth-century millenarianism, although there is hardly a significant discussion of the topic. In short, this book fails to convince the reader that anything of importance has been added to our understanding of Rapp, and Taylor has put the emphasis on Owen in the wrong place.

ROBERT S. FOGARTY
Antioch College

PAUL GOODMAN. *Towards a Christian Republic: Antimasonry and the Great Transition in New England, 1826-1836*. New York: Oxford University Press. 1988. Pp. x, 328. \$34.50.

Antimasons have long puzzled, and sometimes annoyed, historians content to dismiss the Antimasonic movement as an anomalous peculiarity of the early national scene. But Paul Goodman's sweeping revisionist treatment of both Antimasonry and Freemasonry places participants center stage in the drama of America's "Great Transition" from an agrarian to an industrial society. Identifying Freemasonry as the "master symbol" (p. 234) of the massive changes assaulting northeastern communities, Goodman argues compellingly that Antimasons acted as the Minutemen of their generation, alerting neighbors to dangers threatening the Christian republic.

Although much more than an incisive reexamination of Antimasonry, the book is focused on the movement's dynamics and social bases in six New England states. Moving beyond simple polarities of class or religious conflict, Goodman lays bare a complex, heterogeneous base for the Antimasonic persuasion that tapped widespread anxieties about republican political culture, increasing secularism, gender roles, and emergent class consciousness. Freemasonry, Goodman argues, flourished because it aided bourgeois businessmen both in establishing entrepreneurial networks and in distinguishing themselves socially through an elite secret society. Thus, Freemasonry thrived wherever the market economy made substantial inroads, and Antimasonry followed to oppose the fraternity's modernizing tendencies. Freemasonry was latitudinarian, secular in recreational habits and values, elitist, and cosmopolitan, and it flew in the face of domesticity in its all-male culture. It had become a lightning rod for the free-floating anxieties of those who were not so confident about the transforming changes they faced.

Goodman's Antimasons, displacing their concerns onto the lodges that threatened localist, communitarian, and sectarian values, were as middle class and mainstream as their enemies. Many Antimasons were members of the new industrial elite. Their ambivalence and discomfiture with modernization, however, tugged them backward (somewhat like Martin Meyers's reluctant Jacksonians), and they fixed on Antimasonry as a restorationist reform. In their countersubversive perceptions, they echo also the Antimasons portrayed by David B. Davis, but Goodman has identified an enemy at once broader and more concrete. Although Antimasonic perceptions sometimes drifted into paranoia, there were rational grounds. And although ideology and moral outrage came first, the political environment in each state shaped Antimasonry's evolution.

Indeed, Goodman has reconciled disparate threads of interpretation while laying to rest some old bugbears. Antimasonry was not simply a product of the Second Great Awakening; New England evangelicals split sharply between modernists and the antimodernists who fueled religious Antimasonry. Moreover, the author takes both Masons and Antimasons seriously, carefully delineating their ideological and practical wellsprings.

Goodman is at his best in discovering the complex forces that molded New England Antimasonry, as he affirms parts of older interpretations and introduces fascinating new elements, such as Masonry's role in the formation of middle-class culture and the place of republican womanhood in Antimasonry. Gracefully written and wide-ranging, coaxing new insights from traditional materials and tapping new resources (such as Masonic songs), Goodman's argument is multi-layered and trenchant.

Two reservations assure, however, as Goodman anticipated, that the debate will not close on this note. First, Goodman in every instance finds local factors at the heart of Antimasonry, forces rooted in the local history of sectarian relations, economic development, and politics. Thus, while common threads indeed knit together New England Antimasonry, permitting generalization, one must anticipate differences in other locations, particularly those further removed from the "Great Transition." Second, Goodman's interpretation relies on the divergent mass social bases of Masonry and Antimasonry, but here his data (unlike the voting figures, which permit multiple regression analysis) are quite limited. Eschewing the community study and moving to the larger arena of six states, Goodman admittedly sacrifices depth for breadth and relies on small samples, snapshots really, of Masons and Antimasons. Those samples may indeed typify cohorts, but we must wonder until we know much more, for example, about Masons outside of the urban locales such as Boston and Portland or about larger numbers of Antimasons. The concepts of class formation and of the middle class remain sticky wickets, of course. Moreover, political Antimasonry came fairly late to New England and arrived in a unique religious climate that

was strained by late disestablishment and by splits within the Calvinist community. Furthermore, the age of Goodman's anxious Antimasons would not be replicated in other centers of Antimasonry, such as western New York or the Western Reserve of Ohio.

Nonetheless, this book succeeds admirably in its modest aim to begin to resolve recent conflicting findings on Antimasonry, while broadening the agenda considerably. Future students will begin with a much richer tableau thanks to this exciting, rewarding study.

KATHLEEN SMITH KUTOLOWSKI
State University College of New York,
Brooklyn

PATRICK W. CAREY. *People, Priests, and Prelates: Ecclesiastical Democracy and the Tensions of Trusteeship*. (Notre Dame Studies in American Catholicism.) Notre Dame, Ind.: University of Notre Dame Press. 1987. Pp. xii, 362. \$26.95.

Trusteeship, the abuse of the trustee system of property holding that led to serious internal dissensions in several Catholic congregations during the early national and antebellum periods, has been studied in many of its local manifestations, but as a general phenomenon it had never been made the subject of broad investigation until Patrick W. Carey began to publish articles on particular aspects of it. Now in this book he has given us the first comprehensive treatment of the conflicts that occurred between 1785 and 1855 mainly in New York, Philadelphia, Baltimore, Norfolk, Charleston, New Orleans, and Buffalo. In preparation he did extensive research not only in American diocesan and Roman archives but also in collections of pamphlets, broadsides, newspapers, civil court records, and minute books of the meetings of trustees. Instead of recounting one episode after another, he has organized this copious material schematically, drawing evidence from many different cases and examining the nature and structures of the system, its operation, the reasons for its rise and decline, the roles played by the clergy and laity in it, their understanding of their proper function, and the multiple ethnic, social, political, religious, and ideological factors that prompted the outbreaks of hostilities. The result is more than a historical analysis of a peculiar form of discord in the young American Catholic church; it is also an original depiction of the mentalities of the troublesome trustees (mostly laymen with some clerical abettors) and of their opponents.

Carey has found that the strongest influences on the thinking of the trustees derived from the European Enlightenment and American republicanism. Trying to establish a quasi-ecclesiastical democracy in an American national church and to define constitutionally the relative rights and duties of the people, priests, and bishops, the trustees sought complete control over the temporalities, which they extended to the election and dismissal of their pastors and even of their bishops.

In this way they intended to satisfy the need of adapting traditional Catholic institutions and practices to their new political, legal, and cultural situations. It was mainly certain Irish, German, and French (but not the Anglo-American) trustees who heightened the tensions, which were aggravated, if not caused, by the trustees' immigrant backgrounds. Although such public strife rent relatively few parishes, it illustrates anxieties widespread in the church. Sketching a composite picture, Carey shows that the lay trustees came from the propertied, prosperous middle class and represented a majority only of the pew holders and others who contributed enough to be entitled to vote but merely a tiny minority of the congregation at large. Many of them did not practice their faith, and some were even Freemasons. After 1830 such aristocratic leaders began to lose their ascendancy to the centralized authority of the bishops and priests, who were supported by the poor, uneducated, submissive Irish immigrants arriving in ever-greater numbers. The trustees were also submerged by the incoming tide of the universal Catholic Romantic movement fostered by their adversaries. Carey's exploration of the theological sources of the troubles makes clear that the trustees and the anti-trustees were not engaged merely in power struggles but were divided over the very meaning of the Catholic identity in the United States. These few examples of his findings and conclusions suggest how rich the contents of this book are and how rewarding they will be to readers interested in the religious, social, and intellectual history of the period.

ROBERT TRISCO
Catholic University of America

JULIE WINCH. *Philadelphia's Black Elite: Activism, Accommodation, and the Struggle for Autonomy, 1787-1848*. Philadelphia: Temple University Press. 1988. Pp. x, 240. \$34.95.

Philadelphia, the City of Brotherly Love, was the reluctant host to the largest concentration of free blacks in the United States throughout the antebellum period. From 1787 to 1848 the Quaker City was the focus for free black development, and, according to Julie Winch, the community's free black leadership wielded "great influence, both within their own community and far beyond the confines of the city" (p. 1). By 1848, the final year of this study, Philadelphia's blacks made up 5 percent of the city's population. Some twenty thousand blacks, despised and feared by the majority white population, boasted in 1848 of having created nineteen churches, a network of self-supporting schools, benevolent societies, and literary groups.

That vigorous, dynamic black community produced a host of distinguished leaders—men and a few women—over a sixty-year period. "Throughout the antebellum era," Winch observes, "leadership was rooted in a complex network of autonomous Black organiza-

tions, which offered able and articulate men and women within the community a basis for asserting their authority and developing the skills they would require to oversee citywide and, in some cases, national organizations" (p. 2). And yet, ironically, in the city that gave birth to the Declaration of Independence and that boasted of its diversity, sophistication, advanced culture, and great tolerance, white-black relations were no better (and perhaps worse) than in any other American city. All blacks, no matter how intelligent or wealthy, were disfranchised in this enlightened city. Philadelphia's abolitionists barely tolerated their black fellow residents, and ordinary Philadelphians vigorously and forcefully made it clear that they would welcome a black exodus to any place on the globe that would accept them. In the end, sadly, no matter how active, creative, productive, and accommodative the Philadelphia black community became during the antebellum period, its members were no better off than their fellow free blacks in New York, Boston, or any other northern community.

Winch, in her perceptive study, offers the thesis that the leaders produced by Philadelphia's black community belonged to a higher socioeconomic class than ordinary blacks and were among the leadership element for all free blacks in the antebellum era. Leadership, she contends, emerged from the business class and from the clergy. She cites, for example, the illustrious careers of James Forten (sailmaker), Robert Bogle (caterer), and Cyrus Bustill (brewer), as well as the Reverends Absalom Jones and Richard Allen, to buttress her thesis. Those men, and several others, dominated their community as a social, cultural, economic, and political elite from 1787 to 1848. They were instrumental in establishing various religious and cultural organizations that represented most of the community members. They were forceful spokesmen for Philadelphia's blacks.

Winch has honed her doctoral dissertation (begun at Bryn Mawr) into this monograph. In eight succinct chapters, she covers the emergence in the 1780s of a black elite and its response over the years to the many challenges and problems that confronted the community—African resettlement schemes and other colonization proposals, slavery, and a variety of reform movements including abolition. She has read the pertinent literature and offers a useful, if traditional, assessment of the subject. The writing style is clear and concise, though at times a bit labored. All in all, this is a fine example of black community history.

SHELDON HARRIS
California State University,
Northridge

JOHN HEBRON MOORE. *The Emergence of the Cotton Kingdom in the Old Southwest: Mississippi, 1770-1860*. Baton Rouge: Louisiana State University Press. 1988. Pp. xii, 323. \$40.00.

John Hebron Moore, the distinguished historian of antebellum Mississippi, has written a comprehensive account of the rise and maturation of that state's cotton-slave regime to the point of secession in 1861. Despite the dates in its subtitle, the book's coverage of the entire period from 1770 to 1839 is limited to a single opening chapter, far sketchier than subsequent chapters. Thus, the main focus is on the late antebellum period, which is to say that the book covers much well-trodden ground. Progress in agriculture is discussed in five detailed chapters, incorporating research from the author's *Agriculture in Antebellum Mississippi*, published thirty years earlier. In this new presentation, however, Moore advances a more provocative central theme, which is that Mississippi agriculture achieved a peak of "modernity" on the eve of the Civil War, not surpassed until after World War I. The criteria for this claim include the use of farm implements, soil conservation practices, and techniques of managing slave labor. Later chapters discuss activities off the farms without particularly trying to extend this line of argument to the whole of Mississippi society. There is considerable fresh and detailed material on transportation, manufacturing, and the work and leisure activities of white and black town residents.

This is an old-fashioned sort of history, concentrating on contemporary documents and observations, mainly of a nonquantitative character. A partial exception is chapter 6, "Rural Whites," which presents a number of tables based on the agricultural and population censuses for 1850 and 1860. But it does not add appreciably to the information available in existing studies. Because the evidential base for the book is limited mainly to 1840–60, it does not really establish Moore's claim that an agricultural revolution was triggered by the post-1839 depression in the cotton economy. For example, although the author stresses the spread of horse- or mule-drawn implements as a significant new development, it is evident from the 1840 census that large numbers of these draft animals were already in use during the 1830s (p. 297). Similarly, the concept of a "peak of modernity" is defined not by comparison to farming in the North but only in terms of change within the South. Nor does the postwar South receive explicit study. So the book's theme is really only a rough subjective assessment about late antebellum trends. What the book has to offer is an abundance of interesting contemporary examples and illustrations, plus the considered judgments of a man who has immersed himself in these materials for more than thirty years.

These opinions are well worth pondering, but they are often idiosyncratic, and it is difficult to know which ones can be taken as authoritative. It is much easier to present examples or cases of various techniques than it is to show widespread adoption, and the author is not always careful to distinguish one from the other. For example, the observers who enthusiastically described systems of horizontal planting and hillside ditches argued that such practices would "stop the spirit of

emigration" (p. 33) or "save the South" (p. 34), phrases that make it clear that they are railing against prevailing norms.

Thus, when the author argues that the spread of horse- and mule-drawn implements made the gang-labor system increasingly obsolete and that "the spread of the task system throughout the Black Belt" was a major improvement in the lives of plantation slaves in the last two antebellum decades (pp. 95–100), the reader is fascinated but unsure of the status and reliability of these assertions. There are glaring omissions from the bibliography, which means that, on most current issues of debate, the book's contribution is oblique. That considerable evolution and change occurred in the cotton belt during the 1850s is undeniable. The contention that "the people of the southwestern Cotton Kingdom had emerged to become in all respects citizens of the modern Western industrialized civilization" (p. xii) can only be considered an overstatement.

Despite these frustrations, the book has considerable value. The material on towns and on early manufacturing, though less tied to a theme, is rich, informative, and not readily found elsewhere. From the accounts Moore provides of nearly every machine shop and textile mill in the state, one comes away with a reasonable feel for the practical difficulties faced by such enterprises in antebellum Mississippi. The descriptions of life in towns such as Natchez, Vicksburg, Jackson, and Columbus are also detailed and illuminating.

GAVIN WRIGHT
Stanford University

JAMES M. MCPHERSON. *Battle Cry of Freedom: The Civil War Era*. (The Oxford History of the United States, number 6.) New York: Oxford University Press. 1988. Pp. xix, 904. \$35.00.

During the past quarter century Americans have witnessed an extraordinary history parade. Centennial celebrations (sometimes cerebrations) of the Civil War (too rarely, unfortunately, of wartime and post-Apomattox reconstructions) set the cadence during the 1960s. Since then many scholars and funding institutions have shifted attention to the bicentennials of the revolution, the Northwest Ordinance, and the Constitution. Similar celebrations are being planned for the bicentennials of the First Congress and the Bill of Rights. Refusing, however, to abandon the field, historians of the Age of Lincoln, such as James M. McPherson, have paid commendably persistent attention to the Civil War.

This book retraverses terrain McPherson covered in *Ordeal by Fire* (1982), and many readers will find the two books usefully interchangeable. But *Battle Cry of Freedom* attends also to intervening scholarship that illuminates both the war's complex effects on business, gender, law, politics, race, and technology and the reciprocal influences of those and other matters on the

war. Exploiting that front-running research, McPherson strives "to integrate the political and military events of this era with important social and economic developments to form a seamless web of synthesizing up-to-date scholarship with my own research and interpretation" (p. ix).

Does he succeed? Partially and impressively, in ways already praised by reviewers in popular media and by selection committees of a major book club. This book is vigorous, engaging, informative, and emphatic, especially in the substantial segments devoted to politics, broadly defined, and to military actions, actors, and factors. Only Peter Parish's *America's Civil War* (1975) offers in such compact form (yes, seven- and eight-hundred page books on this vast subject are compact) similarly vivid surveys of battles and leaders. McPherson interweaves firefights and logistics, strategy and tactics, and large- and small-unit operations. He communicates the terrors felt by combat soldiers and the garrison boredoms they survived, and he understands the changes in military and naval technology that made the Civil War so deadly and yet, on the Union side at least, so rich in meliorating social organizations and dynamic public policies.

McPherson succeeds less well, however, when he demands more of the past than we yet know how to deliver and, at other times, when he asks too little of its resources. He suggests that precise "legal position[s]" (pp. 315–16) were formulated in the 1860s about permissible limits on government actions in undeclared combat situations, including civil wars. But did not our Civil War illuminate vast imprecisions, and have not Korea, Vietnam, Lebanon, Nicaragua, the Persian Gulf, Irangate, and the ambiguous 1973 War Powers Act suggested how tenacious such uncertainties remain? McPherson criticizes Benjamin "Beast" Butler's "remorseless" (pp. 420, 623–25) military administration of New Orleans. The fact is that abroad, as well as in the United States, the nineteenth century was dolefully rich in martial rulers over urban losers in civil wars. A comparison of Butler with, for example, General Gaston de Gallifet's truly remorseless ways with Paris's *communards* of 1871 or with other analogs from Ireland, Mexico, and China would have profited readers of this book.

McPherson's displeasure with most Civil War "political" generals keeps popping up, as in his suggestion that two Confederate officers skedaddled out of a Union encirclement because they did "not want to surrender their own persons" (p. 401). Such descriptions suggest that, to McPherson, "political" officers displayed defects transcending a lack of military academy education. Yet few high-ranking officers of any period have had greater political misadventures than academy alumni Douglas MacArthur, George McClellan, George Patton, and Oliver North. Further, the author's ambiguities about professional officers' reliance on theorists Karl von Clausewitz and Antoine Henri de Jomini—plus his correct judgment that the Civil War "was pre-eminently a *political* war, a war of

peoples rather than of professional armies" (p. 332)—leave uncertain his estimate of the civilians who leavened academy cadres.

Other uncertainties arise from the author's choice of words and phrases. When considering free labor in nonslave jurisdictions, he states that the wage earner "had only the right to withdraw his labor—to quit and go elsewhere, or to strike" (p. 23). Yet mobility was then such a rare option for ordinary people in most countries that its reality for free laborers in the United States helped lure generations of immigrants to these shores. Further, from 1787 until Appomattox, mobility was almost the only right deriving from American citizenship about which leading legalists concurred, a right not yet perverted by extreme legal positivists into an element of "liberty of contract" doctrine. Indeed, one definition of "involuntary servitude" was legal immobility for bondslaves.

Regarding American slaves, McPherson points to a constraint that greatly offended many contemporaries: the debarment of slaves from the state-defined civil contract of marriage. That exclusion permitted whites to label many black and mulatto Americans as bastards. But McPherson's genteel phraseology almost obscures that fact, one that helped widen fissures between the legal, religious, political, and social cultures of North and South. Surely that issue rates more space than the fact that Lee's beard grew in gray by late 1861.

Phraseology intrudes again when, despite his commendable sensitivity to women's shares in many of the war's tumultuous events, McPherson refers neo-Edwardianly to "women of questionable virtue" (p. 314). Further, in that overwhelmingly rural, equine-dominated age, could only "the sons of the Virginia gentry" (p. 463) have been experienced riders, and did a specialized "troop train" (p. 286) exist in the spring of 1861?

There are still more minor lapses in this book, including typographical errors (on page 398 "bowls" for "bowels" leavens a line) and omitted words. Back-to-back repetition of "grey-eyed man of destiny" on pages 113 and 114 is an instance of overkill. Regarding the notorious Daniel Sickles, a Union political general who in 1859 killed his wife's lover, McPherson accepts the view (one, I fear, that I had shared) that Sickles's able lawyers got him off in the first successful claim of temporary insanity in an American jurisdiction. But Leonard W. Levy (*The Law of the Commonwealth and Chief Justice Shaw: The Evolution of American Law, 1830–1860* [1957]) and John Hughes (*In the Law's Darkness: Isaac Ray and the Medical Jurisprudence of Insanity in Nineteenth-Century America* [1986]) have shown that a Massachusetts court accepted the insanity plea in 1844, resulting in the defendant's commitment to an asylum rather than in his sentencing to the gallows.

Although readers of this book must wait overlong for some definitions (that of "military parolees," on page 636, for example) and fruitlessly for explanations of letters of marque, maritime prizes, and "guns" (that is, cannons), they will nevertheless reap large rewards

from its pages. The lack of comparisons with contemporary civil wars in matters of internal security, diminutions and enlargements of civil liberties, and willingness of a combatant society to risk social improvements in the midst of searing conflicts seems to be the book's major omission, a complaint that suggests how rich feasts lead to greed.

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THOMAS A. LEWIS. *The Guns of Cedar Creek*. New York: Harper and Row. 1988. Pp. xi, 371. \$24.95.

For much of the Civil War, the Shenandoah Valley served two important functions for the Confederate army in northern Virginia. The rich farmlands of the valley provided grain, cattle, and horses for an army whose needs always outran its ability to meet them. The Shenandoah also functioned as a back door corridor to Washington, allowing rebel forces to threaten the Union capital and thus draw pressure off of Richmond. In 1862, for instance, Thomas J. "Stonewall" Jackson was sent into the Shenandoah when George B. McClellan threatened the southern capital. Jackson so scared the Washington authorities that Irvin McDowell's whole corps was held in the city instead of joining McClellan's troops, which weakened the Union offensive. In 1864, with Ulysses S. Grant tightening his grip on the Confederate capital, Robert E. Lee sent Jubal Early to create another diversion in the valley. But at the Battle of Cedar Creek, fought October 19, 1864, Early's army was shattered, and the Confederate use of the area ended for good.

The traditional rendition of the battle is highly dramatic. John B. Gordon, leading the rebel assault, caught the enemy asleep at dawn and in the half light of the early morning drove them from their camps, completely caving in the left wing of the Federals. Philip H. Sheridan, the young charismatic northern commander, had been away discussing strategy with his superiors. But, hearing the sound of cannon as he journeyed back to his command, he made a dashing fourteen-mile ride to rally his troops and turn defeat into victory.

It is basically this tale that Thomas A. Lewis retells. But beneath his smooth prose lies the evidence for a rather modified analysis, one that the author glimpses but does not develop. The initial southern assault was led by Gordon because he had reconnoitered the ground. But then, after the Union troops had been driven from their camps, Early reassumed direction of the army and, under his overly cautious handling, the attack faltered. On the other side, a stiffening Union resistance was directed by calm, methodical Horatio G. Wright, in temporary command during Sheridan's absence. Despite a painful face wound, he remained calm and positioned the confident, unrattled Sixth Corps to reverse the southern tide. Then Sheridan arrived and lent his exuberant energy to the Union

momentum. Because he was fiery and newsworthy, Sheridan got full credit for a victory already in the making.

The author cannot stop to pursue this point because the tenor of Civil War books is set by the traditional need to entertain rather than to inform. To pause and debate Wright's role would break the cadence of the text, which, since the trend-setting work of Bruce Catton, must move forward at a generous rolling pace. Sheridan's ride makes jolly, colorful reading. Moreover, the author approaches the battle by recounting the personal histories of some of the major combatants. Because, surprisingly, Wright does not receive one of these capsule biographies, he perforce remains a relatively undeveloped figure.

One should not be too critical. This is a pleasant book by a professional writer. But the work illustrates the basic problem in too many histories of the Civil War: they are aimed at a distinct popular audience that expects to hear the well-worn tale, like a group of Saxon warriors in the Mead Hall hearing again of Beowulf's deeds, and, therefore, the story cannot be liberated from old forms. Thus, antislavery is still dismissed as fanaticism, and, in this as in other recent books, Afro-Americans are termed Negroes, an indication not only of racial insensitivity but of an inability to break old molds in Civil War writing. Consequently, this book will entertain buffs but will be of little use to scholars in military studies.

MICHAEL C. C. ADAMS
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B. P. GALLAWAY. *The Ragged Rebel: A Common Soldier in W. H. Parsons' Texas Cavalry, 1861-1865*. Austin: University of Texas Press. 1988. Pp. xiv, 186. \$19.95.

The experiences and attitudes of common soldiers during the Civil War have been the subject of recent studies by Gerald F. Linderman, Reid Mitchell, and James I. Robertson, Jr. Those works expand significantly on Bell I. Wiley's pioneering efforts in the field; however, they include scant testimony from soldiers who labored in the obscurity of the trans-Mississippi theater. According little attention to the trans-Mississippi has been a persistent—and understandable—trend in Civil War scholarship: events east of the river were far more dramatic, bloody, and influential in determining the outcome of the war, and written evidence left by soldiers in the trans-Mississippi is modest both in quantity and quality.

In this book B. P. Gallaway recounts the wartime activities of David Carey Nance, who served as a Confederate cavalryman in Louisiana, Arkansas, and his native Texas. Based largely on Nance's unpublished recollections, which were written several decades after the war, and on other family papers, Gallaway's narrative seeks to illuminate Nance's life as well as to provide information on Confederate cavalry operations (especially those of William H. Parsons's commands) in the

trans-Mississippi theater. Students of southern motivation in the sectional crisis will find interesting evidence here. Nance was a deeply religious non-slaveholder who endorsed secession because he resented abolitionist attempts to dictate to the South. He enlisted not out of dedication to states' rights or the South but in a rush of emotion after the stirring ceremony at which Parsons's regiment was organized, and he never formed a significant attachment to the Confederacy as a nation, relying on a sense of duty and loyalty to his peers to keep him in the ranks.

This book evokes, often in Nance's own words, the experience of serving in a backwater region. It is a story of boring camp life, lax discipline, lengthy absences from the front, long rides across a vast territory, illness, hostile weather, and far more attention to the threat of voracious mosquitoes than to seldom-seen Federals. In forty-four months of service, Nance saw action in just a handful of small-scale actions (he was unlucky enough to receive several wounds) before leaving the army after the surrender of the trans-Mississippi in May 1865. Nance functioned within a context of chronic plundering by southern cavalry, casual departures from units without permission, and confusion on the part of officers that suggests the Confederate troops in the trans-Mississippi never really functioned as a meaningful part of the southern war effort.

The routine nature of Nance's service hinders the author's attempt to construct a compelling narrative. Gallaway adds to the problem by overusing clichés. He writes, for example, "The camp they left behind was a beehive of excitement" (p. 30); "Rock-jawed and steely-eyed, they once again prepared to face hostile guns" (p. 89); "Parsons' main force poured from the ravine like ants from an anthill" (p. 95); and "Polignac's boys, occupying the center of the Confederate line south of the fort, fought like tigers" (p. 119). In terms of material on the trans-Mississippi, Gallaway might better have edited Nance's recollections for publication. Yet, despite its shortcomings, this book supplies useful information that certainly will benefit students of that neglected theater.

GARY W. GALLAGHER
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DANIEL E. SUTHERLAND. *The Confederate Carpetbaggers*. Baton Rouge: Louisiana State University Press. 1988. Pp. xv, 360. \$40.00.

While thousands of Union veterans looked to the South as a land of opportunity, thousands of former Confederates looked to the North in the same way. Those former Confederates constituted, according to Daniel E. Sutherland's estimate, at least half of the southern-born who migrated northward between 1865 and 1880. Sutherland has tracked down more than a thousand of them through assiduous research in biographical works and in manuscript collections, of which he lists approx-

imately three hundred. He has gathered enough information on a "core group" of 571 to "draw a fairly complete statistical portrait of the typical Confederate carpetbagger" (p. 2). Much the largest number settled in New York.

But the book is about people as flesh-and-blood individuals, not as statistics. In a clear, readable style, the author relates the experiences of many of the émigrés. He gives by far the greatest attention to Burton N. Harrison, who had been the private secretary of Confederate President Jefferson Davis. Not all of the others that Sutherland discusses served the Confederacy in either a civilian or a military capacity. The famous architect H. H. Richardson, for one, though born in New Orleans, spent the war years mainly in Paris.

Sutherland illustrates a succession of themes through the examples of his subjects. Those people adjusted more or less happily to their new homes. They gained acceptance as northerners by associating with local or national organizations such as the Democratic party but kept their southern identity through membership in sectional organizations such as the United Confederate Veterans and the New York Southern Society. They idealized the Old South, won friends in the North, and strove for reconciliation. They also left a notable legacy in literature, art, architecture, medicine, and other fields. All of this makes an interesting and significant contribution to the history of the period.

But there is a serious reservation about the title of the book. "The appellation *Confederate carpetbagger* may cause some scholars to blanch," the author acknowledges (p. 3). Certainly the appellation would have caused his typical southerner in the North to blanch. True, at the first banquet of the New York Southern Society, "Senator M. C. Butler of South Carolina kidded 'this likely set of carpet-baggers'" (p. 290). But that was in 1887, a decade after the last of the carpetbaggers in the South had fallen from power (and in South Carolina they had fallen as a result of the murderous campaign that the very same M. C. Butler had launched). Many if not most former Confederates in the North left their home states for the same reason that the distinguished scientist Joseph LeConte left South Carolina—because of "the installation of a carpetbagger-scalawag-freedmen government" (p. 20). The refugees from Reconstruction hated the very word "carpetbagger," and, even as late as 1887, a person had better smile when calling one of them that and had better have the anticarpetbagger credentials of an M. C. Butler.

The author defends his persistent and anachronistic use of the term by saying that postwar northerners went South to improve their condition, and "Confederates moved north for the same reason" (p. 3). Yes, but the majority of the northerners who went South were not called carpetbaggers; only those were so called who participated in politics as Republicans and helped their party temporarily to dominate. The former Confederates, on the other hand, "had little chance of

... winning political success in the North," as the author himself points out (pp. 100–01). "They had no Reconstruction Acts ... to aid them in shaping political careers." Exactly! That made them fundamentally different from the real carpetbaggers, the Republicans from the North who, under the Reconstruction acts, shaped political careers for themselves in the South.

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ERIC FONER. *Reconstruction: America's Unfinished Revolution, 1863–1877*. (New American Nation Series.) New York: Harper and Row. 1988. Pp. xxvii, 690. \$29.95.

Scholars, teachers, and students of American history stand again in debt to Eric Foner. By now we are familiar with the depth, reliability, and comprehensiveness of his earlier works, especially as they have illuminated the politics and aspirations of nineteenth-century American life. In this latest contribution to the recollection of our national experience, Foner has provided a thoughtful, carefully researched and clearly written survey of the Reconstruction period.

Early in the book Foner states his intention to build on the extensive postdepression American scholarship in this once explosively controversial field. He seeks "to provide ... a coherent, comprehensive, modern account of Reconstruction ... to view the period as a whole, integrating the social, political and economic aspects of Reconstruction into a coherent, analytical narrative" (pp. xxiv, xxvii). Within that broad context, he attempts to develop several unifying themes—among them, "the centrality of the black experience" in Reconstruction, the larger American context of "the emergence of a national state," and exploration of the ways in which the social, economic, political, and moral development of the North affected the course of Reconstruction in the South (pp. xxiv–xxvi).

For those readers who are acquainted with the trenchant revisionist approach to Reconstruction opened by W. E. B. Du Bois's *Black Reconstruction* (1935), a debt that Foner gladly acknowledges, and who have followed Reconstruction scholarship since then, Foner's main lines are not new. He offers, however, such rich details and informed observations that we are constantly moved toward important new insights and connections.

Essentially, then, Foner has succeeded in his basic purposes. His long-standing interest in the rise of free labor ideology in America, the early development of the Republican party, and the critical role of black people in the creation of the American nation have all been marshalled to guide his meticulous research in an impressive variety of sources. As a result, he provides us with a full and often complex sense of the persons, ideas, institutions, and events in the northern and southern sectors of the nation that helped account for the vast hope, the important successes, and the eventual great failure of Reconstruction.

Throughout the work, as he focuses on the significance of Reconstruction for such matters as education, race relations, the labor system, and national economic development, Foner is sensitive to the fact that something more was always at work. Indeed, he reminds us that Reconstruction cannot be understood unless we see that its "most striking ... impact" was on the freed black people (p. 410). They were the often hidden heart of the affair. Simply to know that is to share a great human solidarity, a necessary gift for those who would evoke the spirit of grand experiments in social transformation.

Only in the context of Foner's important achievement may it be helpful to address certain weaknesses and ambiguities in his work. One ambiguity was surely inescapable, for the vexing question of the white Republican party's commitment to Reconstruction's experiment in democracy (to use Du Bois's phrase) and to its beleaguered black party members is deeply woven into the fabric of the entire Reconstruction experience, continuing in other forms into our own time.

Foner's treatment of Republican "commitment" to Reconstruction tends to slip into uneasy contradictions. At the end of the 1860s, Foner writes, "Nearly all Republicans still believed Reconstruction must be defended. But it became increasingly clear that calls for further efforts to promote social change in the South stood little chance of success" (p. 51). When we read in Foner's careful account of the ways in which northern party leaders treated southern, especially black, Republicans as poor, bothersome relatives, we are forced to ask just what "Reconstruction" those Republicans planned to defend—and when? Moreover, what did it mean to "defend" Reconstruction when the party tired of working for some kinds of social change but eagerly promoted others, such as the development of American expansionism and the building of great financial empires?

Surely the defense of Reconstruction demanded continuous struggle for change, including the intervention of the federal government on behalf of the endangered, newly claimed citizens of African descent. So we must ask Foner to reconsider what he means when he writes, "While unwilling to push further with Reconstruction ... party leaders remained committed to what had been accomplished" (p. 452). Was that a contradiction in terms? Is that not the point of the book's subtitle?

A related problem in the book is its structure and balance. Foner seeks to take the story to the Compromise of 1877, the conventionally understood point of no return. The book is organized, however, so that three-quarters of it covers the period up to the early 1870s. The rest of the story, North and South, is squeezed into the remaining quarter. As a result, Foner tends to undermine his own commitment to demonstrating the centrality of the black experience in Reconstruction. For one of the most compelling, and largely unexamined, aspects of Reconstruction is the rise of the southern black Republican force in the post-1870

period. As Foner notes in his relatively brief treatment of that time, "It did not take long for black leaders to become dissatisfied with the role of Junior Partners in the Republican coalition" (p. 352).

So it was in the period of Ulysses S. Grant's first term, during the rise of the Liberal Republicans and in the heart of the Panic of 1873, that powerful expressions of "black assertiveness" arose as Afro-Americans struggled to challenge white Republican hegemony in the southern states that remained under Republican control. Foner's decision to condense the events of this period, after having so amply (at times perhaps too amply) told the earlier story, robs us of exploring with him this part of the Reconstruction experiment. Indeed, only in the light of that struggle can we understand why so many northerners, including Republican leaders, so readily accepted the betrayal called "Redemption," "Reconciliation," or "Home Rule"—all very white in intent and thus far less threatening than a continuing struggle for black political equality, for a bold experiment in democracy. To miss the power of that social and political dynamic is to miss the richest meaning of Foner's own conclusion: "For the nation as a whole, the collapse of Reconstruction was a tragedy that deeply affected the course of its future development" (p. 604).

Nevertheless, anyone who eventually seeks to tell the story of that short-lived but crucial spurt of hope and desperate audacity in the 1870s will have to be guided by the model of Foner's rich and comprehensive use of his sources and his imagination. He has taken us deeply into the black and white experience of Reconstruction and has opened valuable insights for our understanding of both the post-Civil War nation and our own ambiguous times. Any historian who is able to do that is a gift to the nation and to the profession.

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TENNANT S. MCWILLIAMS. *The New South Faces the World: Foreign Affairs and the Southern Sense of Self, 1877-1950*. Baton Rouge: Louisiana State University Press. 1988. Pp. viii, 165. \$22.50.

In his study of the New South and foreign affairs, Tennant S. McWilliams raises a central question: why have southerners failed to develop a realistic attitude about U.S. relations with the rest of the world? He notes that throughout their history southerners have encountered failure, poverty, guilt, defeat, and ridicule and that their experiences seem at odds with the notions of innocence and invincibility that have fueled the flames of American idealism. Yet McWilliams points out that southerners have joined with northerners in accepting the idea of a mission to extend the American way of life to people around the world. Thus, he asks, what happened between the end of the Civil War and the beginning of the cold war that can

help explain the failure of realism to dampen the crusading spirit in the South?

In attempting to answer this question, McWilliams explores the relationship between the New South movement and American expansionist sentiment. His analysis focuses on white middle-class southerners who favored progressive change in their region. To illustrate the evolution of southern attitudes toward world affairs, McWilliams offers five case studies of New South leaders. He aims to show that different generations of New South advocates sought to inject something positive from their own sectional experience into the shaping of U.S. foreign policy.

In his case studies, McWilliams advances the following argument: the invasion and occupation of the southern states during the Civil War and Reconstruction era led most early New South proponents to respect the principle of self-determination and to reject on moralistic grounds the idea of American expansion. But this anti-imperialist outlook quickly disintegrated, and before long southerners sought sectional reconciliation by joining with northerners in advocating national expansion. New South businessmen trumpeted the idea that an expansionist program would not only stimulate industrial development in their part of the country but also spread the blessings of civilization around the globe. But southerners felt far removed from the centers of national power until Woodrow Wilson entered the White House. Their deep desire for a positive self-image led them to regard Wilson as a southerner and to view his crusade for the League of Nations as an indication that their section had returned to prominence. In line with their belief in the doctrine of white supremacy, moreover, New South progressives advocated Anglo-Saxon leadership in an effort to establish a peaceful and prosperous world order. But New South liberals abandoned their commitment to the campaign for international cooperation as Russian-American tension heightened after World War II. No longer able to achieve regional self-esteem by identifying with Wilsonian idealism, they felt compelled to seek sectional redemption by resorting to ethnocentric nationalism and embracing a cold war outlook.

There are two basic weaknesses in the argument advanced in the five case studies. In the first place, McWilliams fails to make a clear distinction between demands for territorial aggrandizement and desires for commercial empire. He points out that many early New South advocates opposed the acquisition of distant territories, but he overlooks the fact that postbellum southerners generally supported the drive for overseas commerce. If McWilliams is correct when he claims that James H. Blount was not interested in opening foreign markets for southern products, then McWilliams is wrong when he portrays Blount as a typical New South leader during the Gilded Age. In the second place, McWilliams fails to give adequate attention to the relationship between economic self-interest and southern support for Wilsonian goals. The primary documents reveal that most New South liberals

believed international cooperation would benefit the economy of their region. Southern historians might be intrigued by the contention that New South leaders revered Wilson for psychological reasons arising from their unique sectional experiences. But diplomatic historians will be disappointed to find that McWilliams gives short shrift to the traditional export orientation that helped shape southern attitudes toward American foreign policy.

PATRICK J. HEARDEN
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DEWEY W. GRANTHAM. *The Life and Death of the Solid South: A Political History*. (New Perspectives on the South.) Lexington: University Press of Kentucky. 1988. Pp. xiv, 257. \$25.00.

Dewey W. Grantham offers a survey of southern politics from Reconstruction to Ronald Reagan. At the outset, the author confronts the daunting guidelines often mandated by editors of multivolume ventures: to address a heterogeneous group—students, scholars, and general readers—and cover a complex topic in two hundred pages.

Within these constraints Grantham has produced a workmanlike synthesis of the conventional wisdom as delineated by a plethora of doctoral candidates and monographic authors. His own research has focused on southern progressivism, and he is frank to admit that, of necessity, he has relied heavily on the work of other historians, political scientists, sociologists, and journalists who have delved into other aspects of his broad topic. If his compendium reveals certain weaknesses, therefore, the blame would appear to rest not so much on Grantham as on the scholarly and journalistic professions in general.

Shortcomings are more apparent, naturally, in the coverage of southern political history since World War II, an era that scholars have only begun to examine. For example, a clearer depiction of the basic intent and leadership of the states' rights or "Dixiecrat" revolt of 1948 will surely emerge from further scrutiny. Grantham vaguely describes the instigators of that tactic as "a group of the more rebellious southern Democrats," although he does observe cautiously that "some southerners supported the States' Rights Democrats because of their hostility toward New Deal economic policies" (p. 122).

The career of George Wallace offers another topic thus far only lightly investigated by scholars. Detailed study of Wallace's gubernatorial administrations, tax policies, and hidden allies is needed before historians reach the definitive conclusion that this Alabamian was, in Grantham's words, "in some respects a latter-day Populist" who "believed in using the powers of government to assist lower-income people" (p. 172).

Grantham desists from pointing out recurring themes, such as, for example, the similarities in the use of racism and clever election maneuvers and in the

bourgeois leadership and reactionary goals between the rebellious Democrats of 1948 and the disfranchisers of the early twentieth century. He describes the post-Civil War Redeemers as identifying themselves with "the larger white community in terms of cherished southern myths and cultural values" and as being perceived to represent the interests of most white southerners (p. 23), but he stops short of noting how well this description fits modern-day Republican leaders of the South.

I deplore a tendency on the part of many of the academics on whose work Grantham has relied to accept blatantly political ploys and labels—such as "States' Rights party"—at face value. For example, further study may make it clear that use of the ancient and generally discredited "interposition" doctrine in the early 1950s was mere grandstanding rather than, in Grantham's words, seemingly an offer of "a marvelous strategem to turn back federal intervention in the South's traditional pattern of race relations" (p. 139). The "Southern Manifesto," to cite another example, may well be shown to have been a windy piece of propaganda intended primarily for consumption back home rather than "a dramatic indication of the South's mounting political defiance" (p. 138).

The self-styled Citizens' Councils also invite further study, particularly of the identity of leaders of this effort to devise a respectable version of the Ku Klux Klan. Grantham confines himself to quoting Pat Watters's ingenuous amazement on finding that the Citizens' Councils' efforts were supported in one small southern town not only by the "semi-literate poor white gas station attendant" but also by numerous bankers, politicians, editors, and preachers (p. 136). How, one might well ask, did Watters happen to give that hapless gas station attendant (actually a victim) top billing in his litany of villains? Even continued use of that convenient term "Solid South" may perpetuate an inaccurate impression of a region whose history over the past century has been marked by internal political insurgency, intraparty battles, and widespread disfranchisement.

Grantham's digest of the current orthodoxy on southern politics, supplemented by a lengthy bibliographical essay, provides a useful survey for students and general readers. Its shortcomings illuminate the challenges facing those scholars who probe the Byzantine structure of southern politics: the need to delve deeper, and with a healthy dose of skepticism, into the recent past and the responsibility to practice the demanding art of analysis, in the tradition of V. O. Key, based on familiarity with the broad historical canvas.

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ALTINA L. WALLER. *Feud: Hatfields, McCoys, and Social Change in Appalachia, 1860–1900*. (Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of

North Carolina Press. 1988. Pp. xiii, 313. Cloth \$32.50, paper \$12.50.

In a recent country song, "Readin', Rightin', Rt. 23," Dwight Yoakam sings of Appalachian migrants: "Have you ever been down Kentucky way, say south of Prestonsburg, / Have you ever been up in a holler, have you ever heard, / A mountainman cough his life away, / Digging that black coal in those dark mines, those dark mines? / If you have, you might just understand, / The reasons that they left it all behind" ("Readin', Rightin', Rt. 23," written by Dwight Yoakam, ©1987 Coal Dust West Music [BMI]). Altina L. Waller's book is on a growing list of scholarly works that helps us understand the transformation of the mountains and their people, indeed, the historical processes that led to the exodus along Route 23. Waller's mountaineers began as subsistence farmers, and many of them ended up, like Yoakam's miners, victims of extractive industries. The famous Hatfield-McCoy feud of the 1880s is Waller's point of departure in this fine book. She traces the origins, course, and results of that bloody series of incidents with greater thoroughness than any previous scholar. More important, she uses the feud as a window on the transformation of the mountains.

Waller begins with the Civil War. The Tug River Valley, located on the Kentucky–West Virginia border was strongly pro-South, not because there were slaveholding interests to defend but because the Confederacy seemed to promise more personal, social, and economic independence than the federal government. The North's victory did not change much in the mountains, and certainly the feud was not, as some have argued, a continuation of the war, writ small. As the century waned, however, the Tug Valley, like other parts of Appalachia, grew too small to sustain its ever-growing families; fathers no longer owned sufficient land to pass on to their sons, and the mountains offered no new tillable soil. At this moment, some residents began to realize that the woodlands were valuable for their timber. One such man was William Anderson "Devil Anse" Hatfield. Through a series of litigious, aggressive, and rather unneighborly moves, he began to acquire thousands of acres of land and to employ a few dozen men as loggers. As Waller shows, this situation was filled with tension, for disparities of wealth and power pitted entrepreneurs against wage laborers, subsistence farmers against those in the market place. Increased competition meant increased hostility, and men like Devil Anse were accused of violating community mores to take unfair advantage of others' misfortunes.

It is against this backdrop of social and economic change—decline of subsistence agriculture, the rise of wage labor, the amassing of wealth, increased dependence on nonlocal markets—that Waller locates the revenge killing of three McCoys by Devil Anse and his supporters (the three had murdered Anse's brother). The alignments on both sides were only loosely based on kinship; McCoys supported Hatfields and vice

versa. The real fault lines involved economic self-interest and ideology, exploiters of timber lands for markets versus more traditional folk. But all of these were locally oriented people, "white savages" in the eyes of the coal and railroad executives, the urban journalists, and the state and local politicians who would soon exploit the feud for their own purposes. Before it was over, several more Tug Valley residents were dead, this time at the hands of government officials. West Virginia and Kentucky pointed accusingly at each other, the image of bloodthirsty mountaineers was fixed in the public mind, and northern companies were free to exploit the Tug Valley, leaving a bit behind for locals who cooperated.

The fundamental point Waller makes so forcefully is that the Hatfield-McCoy feud was not some sort of age-old blood-rite of the mountains, a particularly grim example of Appalachian traditionalism. Rather, it was the conflict of local entrepreneurial ambitions and more traditional social ideals that generated the early stages of the feud, and it was the entrance of outside capital (railroad and coal companies) and of state and local government taking sides in the initial dispute for the purpose of gaining control of the region's resources that led to the feud's bloody end.

Along the way, Waller does a fine job of respecting local detail yet setting her story in the larger national context. She introduces us to several fascinating characters, walks us through the terrain, and gives us a sense of the workings of Tug Valley institutions in this personalistic environment. People and place act and are acted on; complexity of motivations never gets lost here. Thus, the great forces and events—industrialization, the penetration of northern capital into the mountains, the rise of a local entrepreneurial elite, the consolidation of state power—do not simply roll over everything in sight but, like a river, follow a course dictated by local topography.

A few things might have been done a bit differently. Waller tells her story well, but she occasionally is repetitious. More important, she sometimes romanticizes preindustrial mountain life. I suspect that in her properly placed desire to show how the new social alignments encouraged bloodshed, she plays down the violence of mountain culture in an earlier era. Waller's argument that mass killings arose out of new scrums of wealth and power is in no way diminished by the generations-old tendency of many Appalachian men to settle personal disputes with tooth and nail, gun and knife. Finally, it would have been nice to have a brief discussion of why Americans have so willingly embraced the false myth of the Hatfield-McCoy feud. Why has the image of moonshining, lazy, and bloodthirsty mountaineers been so popular? Why does contemporary America need "The Dukes of Hazard"? Despite these quibbles, this is a fine book and deserves a wide readership.

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PETER B. HALES. *William Henry Jackson and the Transformation of the American Landscape*. Philadelphia: Temple University Press. 1988. Pp. xii, 355. \$39.95.

William Henry Jackson spent most of the summer of 1873 searching the western slope of the Colorado Rockies for the Mount of the Holy Cross. When he located it, he made the first photographs of the snow-filled crevasses that did indeed resemble the paramount Christian icon. But in subsequent years Jackson retouched his negative: "improving" one of the arms of the cross, altering the shape of an ice field to suggest a praying angel, and even moving a distant creek to a front and center location at the foot of the mountain. What concerns Peter B. Hales in this handsome book is an explanation of those kinds of landscape transformations. He is interested in how photography both reflected and influenced changing American ideas about the West.

This book is not a biography of Jackson. Hales knew that several good ones existed, including Helen Markley Miller's *Lens on the West* (1966) and Beaumont Newhall and Diana E. Edkins's *William Henry Jackson* (1974). Nor is this a history of photography in the manner of Weston Naeff and James N. Wood's *Era of Exploration: The Rise of Landscape Photography in the American West, 1860-1885* (1975). Instead, Hales uses Jackson's life and thought as a mirror in which to see changing American ideas about an environment that was itself undergoing rapid change.

That is a useful approach. Jackson lived from 1843 until 1942, surely the critical century for the disappearance of the American frontier. Hales wades into the often-complex currents of that subject with considerable vigor. Through photographs (193 fine black and white reproductions grace the book) and words, we see the West as both escape from civilization and threshold of civilization. We see it as chaotic wilderness and morally redeeming sublimity. But sometimes we are given a smudged lens. The author's efforts to be profound are occasionally so convoluted as to require third and fourth readings. And the reader wonders if Hales is "pushing" more significance into the details of Jackson's photographs than the artist himself would attribute to them.

The pinnacle of Jackson's career was undoubtedly his exploration of Wyoming's Yellowstone country in 1871 as part of Ferdinand B. Hayden's U.S. Geological Survey of the Territories. There had been several earlier reports of that remarkable region, but Jackson—along with his friend, artist Thomas Moran—provided the first visual documentation and contributed to congressional designation of Yellowstone as a national park in 1892, at which time there was substantial evidence that the park was already being loved to death by tourists.

Considering the sustained American anguish over the vanishing frontier since Frederick Jackson Turner broached the subject in 1893, the reader of Hales's book looks for a report of Jackson's ideas. Surprisingly,

his sympathies seem to be almost entirely with advancing civilization. Indeed, Jackson's later commercial photography advertising western scenery helped bring an end to the very conditions that explain his, and his nation's, uniqueness.

RODERICK FRAZIER NASH
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MARKKU HENRIKSSON. *The Indian on Capitol Hill: Indian Legislation and the United States Congress, 1862-1907*. (Studia Historica, number 25.) Helsinki: Suomen Historiallinen Seura. 1988. Pp. xviii, 325.

This seminal and provocative monograph deserves wide attention, for it is the first study ever to confront and synthesize the issues of land policy, law, and Indian affairs in the U.S. Congress during the critical period of western expansion between the enactment of the Homestead Act and the full implementation of the Curtis Act, which nullified tribal governments and landholdings in the Indian Territory. Contrary to what Markku Henriksson considers to be historical misconceptions fostered by scholarly neglect of the role of Congress, his extensive review of the primary sources has led him to conclude that the congressional representatives of this period were relatively well informed about Indian matters, that they well understood the close connection between land legislation and Indian policy, that they assumed and maintained the dominant role in planning and making such policy, and that they actively guided and directed westward expansion in advance and in anticipation of settlement.

Henriksson asserts that the common intellectual foundation for the formulation of Indian policy throughout this period lay in the almost universal belief among legislators in the so-called linear theory of development, which assumed that different cultures were related to each other hierarchically. Even the Indians' staunchest defenders on Capitol Hill tended to proceed from the idea that Indians were an inferior race doomed to extinction if not saved by the dominant and superior white culture. In examining the legality and impact of Indian legislation, the author concludes that all of the laws affecting tribes amounted to forced legislation based on ambiguous legal grounds and antithetical to the nation's founding principle of government by consent of the governed.

This book is an important and stimulating work that fills a void in the historiography of Congress, land policy, and Indian affairs and should therefore be read by all who profess an interest in those subjects. The breadth of its coverage, when combined with the brevity of its presentation, its frequent historiographical discussions, and its appearance in first edition as a paperback, also lends itself well to classroom use. The volume is based on sound research and is relatively well written, although the text suffers from numerous typographical errors and a few awkward usages that

might be attributable to the fact that the author and the publisher were not working in their native language. Henriksson is a Finnish scholar, and this work is a revision of his doctoral thesis written at the University of Helsinki, which was the first dissertation ever accepted in Finland on a topic related to American Indians.

The conclusion drawn from this study is that despite the legislators' ideas about social evolution and their general desire to help Indians, their first loyalty was to their constituents. Thus, the Congress enacted laws beneficial to white voters back home, even when congressional representatives knew that Indians would suffer as a result. In the final analysis, the legislators' simplistic conceptualizations did not fit, and federal policies failed because they were based on the false premise of the universality of linear development. In Henriksson's view, the decline of the Indians "was foreordained by this failure of Congress, not by Manifest Destiny or any natural order" (p. 286).

MICHAEL L. LAWSON
Bureau of Indian Affairs

GLENDIA RILEY. *The Female Frontier: A Comparative View of Women on the Prairie and the Plains*. Lawrence: University Press of Kansas. 1988. Pp. x, 299. \$25.00.

In this book Glenda Riley offers a detailed and carefully structured description of women's lives on the prairies and plains of the United States from about 1815 to 1915. Demoting Beret Hansa from quintessential frontierswoman, Riley documents marital, racial, ethnic, religious, and class variations among the women. To counter "the customary image of plainswomen . . . as only domestic creatures laboring diligently and without pay alongside their men in family enterprises" (p. 147), Riley examines women's various economic roles inside and outside the home. Finally, she shows how some women expanded their spheres to encompass various community activities. Based on her findings, Riley argues that, while a "physical setting and its resources" shaped men's lives, gender was the "key factor" in women's lives (p. 2).

Much in that argument is irrefutable. Riley has carefully combed the manuscript and printed sources, and the information alone is a valuable contribution to the history of western women. Yet the argument is not entirely persuasive. Riley defines women's roles so broadly that the notion of roles loses some of its meaning, and her interpretation may accommodate so much variation that it loses some of its explanatory ability. For example, Riley argues that, even when women held paid employment outside the home, such work did not constitute different experiences because the jobs open to women generally were simply commercial versions of women's household tasks. In other words, whether making clothes at home or as a paid seamstress, a woman's work was generally the same. Although that is technically correct, the two work

experiences are both economically and psychologically different. Gender roles so broadly defined could also lead to the argument that men's lives are basically the same, since their gender roles require them to be in charge of the home, to earn a living, and to be responsible for the public life of the family. Whether men support their families by farming, mining, or ranching, they could still be seen as following the dictates of their gender definitions.

As a final point in her book, Riley argues that "women's shared responsibilities, life styles, and sensibilities constituted a female frontier" and that by attending to this concept historians will be able to write a story of "women's westering . . . as complete as that of men's" (p. 200). In fact, however, her work suggests an even more provocative conclusion: that the concept of region itself needs reevaluation. Region serves as a basic organizing principle in U.S. history. Southern and western history, especially, presume the centrality of region for understanding people's lives, politics, values, economics, and interests. But Riley's work flatly contradicts that presumption. When she writes, "While the mineral, soil, timber, or other resources of a region highly influenced men's occupations, activities, and ideas, they marginally affected those of women" (p. 3), she is really saying that the constituent elements of what makes a region a region are largely irrelevant to women's lives. If she is right, it must follow that region itself is of marginal value in explaining either women's experiences or the country's past and must be reconsidered. That conclusion calls for nothing less than a fundamental reordering of U.S. history.

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BEVERLY BEETON. *Women Vote in the West: The Woman Suffrage Movement, 1869-1896*. (American Legal and Constitutional History.) New York: Garland. 1986. Pp. xii, 164. \$30.00.

Any study of the woman suffrage movement in the United States must deal with the important geographical fact that the only states to award the vote to women in the nineteenth century were those Rocky Mountain neighbors Wyoming, Colorado, Utah, and Idaho. It is surely not coincidental that contiguous states granted suffrage to women so early. Why then did woman suffrage come first to that particular area? Beverly Beeton, in this book, deals directly with that question.

Carefully examining the process of granting votes to women in each of those territories and states, Beeton is able to draw some important conclusions. She shows, for example, that suffrage was considered and granted not out of adherence to an ideology of equality, nor because of an effective movement by women, but because of pragmatic political reasons. Suffrage is, after all, a political issue, and Beeton deals very effectively with the relationship between woman suffrage

and organized political parties in the West. The case of the Populists is the most interesting, since they were so important for the suffrage victories in Colorado and Idaho, but Beeton is also careful to illustrate the relationship between the suffragists and the two major parties. Equally important, she shows that once the vote was achieved, women did not attach themselves to any particular political group, nor did they adhere to any particular political philosophy. Thus, she finds that at the very time their sisters in the East were predicting that woman suffrage would bring about moral reform, voting women in the West were engaging in politics as usual.

Wyoming, Colorado, and Idaho each receive separate chapters in the study, but Utah claims most of Beeton's attention—fully half of the book. Most important is Beeton's cogent explanation of the relationship between the suffrage issue and Mormonism in Utah and, to a lesser extent, in Idaho. The Mormon hierarchy supported woman suffrage from 1870 on, so suffragists were placed in the uncomfortable position of either having to agree with church leaders, thus risking condemnation because of the polygamy issue, or, what seemed worse, opposing votes for women in Utah.

Finally, Beeton considers the question of why the early successes in the West were followed by fourteen years of failures. She discusses the mood of the country, the changes in the suffrage movement, and the rising opposition not only from liquor interests and practicing politicians but also from women opposed to suffrage. All of those factors kept the western example from spreading, even while citizens from the West were gladly testifying to the success of the experiments in their states.

The book is not without flaws. For example, there is no index, which would have been especially useful in a book with such a wealth of detail. In addition, the emphasis on Utah at the expense of the other states, especially Wyoming, can be questioned. Those, however, are minor issues in an otherwise excellent work that draws together material that has, for the most part, been dealt with on a state-by-state basis in the past. Beeton's search for patterns and significance in these western states provides the reader with valuable insights into both the history of the woman suffrage movement and the political history of the American West.

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LARRY M. LOGUE. *A Sermon in the Desert: Belief and Behavior in Early St. George, Utah*. Urbana: University of Illinois Press. 1988. Pp. xii, 165. \$19.95.

The title and the five chapter headings of this brief survey seem to promise a kind of local social history. What Larry M. Logue has really given us is an excellent little demographic study of one small, typically cohesive

Mormon community. Although a stream of books about the Mormons have been tumbling off the presses, this is the only recent empirical study of Mormon society during the last third of the nineteenth century. It is a very good one.

Logue's contribution lies in his tossing aside some old chestnuts. One is that polygamous husbands fathered fewer children than monogamous men; actually, the numbers were about equal. A second, namely, that big families were economic assets, is also discarded, at least for Utah. Logue shows convincingly that numerous children were in fact burdensome and that economic life suffered from the underemployment of sons. Third, he corrects two long-accepted generalizations about polygamy: the notion that polygamous families probably never amounted to much more than 10 percent of Mormon marriages (by analyzing the statistics and primary sources more carefully than other scholars have, Logue is able to show that as many as one-third of the marriages were polygamous); and that there were peaks and valleys in the rate of polygamous marriage (Logue finds that frequency declined only under the threat of federal raids).

Logue is less successful in his two larger, nondemographic goals. He simply does not deliver on his promise to show that the reason for studying local communities is not to test received theory but "to generate theory" (p. xi). But then how many historians have ever succeeded in that sort of enterprise? He does make some headway toward another goal: his attempt to explain what an ordinary Mormon had to gain by remaining in a church that was constantly at war with the dominant society and whose official doctrine could be seriously at odds with popular everyday Mormon religion. The Mormon community, he finds, remained loyal to the official church, not only because the community venerated authority but also because popular religious practice was able to modify official norms. Such departures from official doctrine were "important ways believers [made] religion work in difficult times" (p. 17), providing a bulwark of "self-determination" against "authority" (p. 19). The official doctrine taught, for example, that the first period of the afterlife was one of intense activity, but popular religion changed that to a period of rest, with important implications for attitudes toward death. The nonquantitative parts of the book are, unfortunately, too short. If expanded and less dependent on secondary works, they would have rounded out and refined several related studies of the Mormon preoccupation with marriage, family, and the afterlife by Leonard J. Arrington, Davis Bitton, and Klaus Hansen.

But we should be grateful to Logue for what he has accomplished. The book is not just another demographic community study; it is a good piece of history.

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ROBERT C. OSTERGREN. *A Community Transplanted: The Trans-Atlantic Experience of a Swedish Immigrant Settlement in the Upper Middle West, 1835–1915*. (Social Demography.) Madison: University of Wisconsin Press. 1988. Pp. xv, 400. Cloth \$45.00, paper \$19.95.

Robert C. Ostergren has painted a compelling portrait of the cultural development of two Swedish communities connected by a transatlantic link of migration. His book is another in a growing number of studies that examine both sides of the migration process. Ostergren first analyzes the social and economic development in Rättvik, a parish in Dalarna, Sweden. He then traces the immigrants from there to the two central Rättvik-American colonies in Isanti County, Minnesota, and Clay County, South Dakota, that underwent a complex process of cultural retention and change. Behavior in the two societies repeatedly underscores Ostergren's basic point: among the emigrants, the migration to America was a conservative movement, an attempt to preserve an old society in a new milieu.

Peasant society in early nineteenth-century Rättvik was a homeostatic regime controlled by strictures on marriage and patterns of property devolution. Unlike much of Sweden at the time, the region practiced partible inheritance, which permitted most in the society to acquire the land necessary for family formation. Periodic enclosures of land consolidated the estates of the freeholding peasants and actually restored the household head's ability to subdivide land and benefit his or her children. Yet the population continued to grow, and a series of economic jolts weakened the society. By the time of massive crop failures in the late 1860s, the rate of emigration, just underway, quickened.

The early emigration of families was predicated on an effort to preserve social status that was endangered at home. Using a wealth of Swedish source material, Ostergren carefully reconstructs the process of emigration diffusion. The leaders of the migration tended to be wealthier and more literate and were embedded in influential kin networks that were disproportionately affected by America fever. In this classic pattern of family-oriented chain migration to the American Midwest, then, the immigrants were not motivated solely by economic gain; rather, they attempted to preserve and reestablish a familiar social order that included economic security.

Given those motives, the immigrant community was isolated and conservative. Ostergren maps the patterns of land taking, which in some cases resulted in a near-complete transplantation of residential propinquity from Sweden to America. Moreover, administrative units of the local church, the central immigrant institution, not only replicated church organization in Sweden but also continued to distinguish membership in the units according to former places of residence in the mother parish. In short, community organization was often older than the community itself.

The immigrant community, however, was not imper-

vious to change. Agricultural patterns shifted as immigrants planted unfamiliar crops, such as wheat, that provided them an important source of income. Immigrants accepted American linguistic and political traditions. Yet, at least until 1915, they inwardly remained distinct from the host society. Swedish-American patterns of fertility, inheritance, and geographical mobility continued to indicate a conservative desire to maintain old patterns rather than to become immersed in the market economy.

Ostergren underscores the immigrants' success in preserving their social fabric in the United States by returning to postmigration Rättvik. By 1900 the old homogeneous community had been "fractured" (p. 304). The population was undergoing increased polarization, proletarianization, and out-migration. Compared to life in Sweden, the isolated Swedish settlements in the United States had become "fossilized" (p. 328). The irony is thus complete: the immigrants who ripped themselves out of their peasant community had experienced less disruption than those who stayed behind.

Ostergren's study is an example of historical geography at its best. In this book, lavishly illustrated with figures and photographs and densely detailed with tables, Ostergren intricately portrays such observable behavior as land use, demography, and spatial diffusion. This is also a fine case study, and Ostergren carefully places the sending and receiving communities in the larger context of Swedish and middle western American society. The major weakness is that, although a wealth of evidence reveals clear patterns of behavior, it does not explicitly address the motivations for the behavior. Since few narrative sources are consulted, motivation in many cases must be intuited. Any criticism, however, ought not detract from Ostergren's accomplishment. Through careful research and engaging prose, Ostergren has produced a study that immeasurably enlarges our understanding of the preindustrial immigration that connected European and American societies.

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JERRY W. CALVERT. *The Gibraltar: Socialism and Labor in Butte, Montana, 1895–1920*. Helena: Montana Historical Society Press; distributed by University of Washington Press, Seattle. 1988. Pp. viii, 189. \$21.95.

If the landscape of Debsian socialism calls to mind the factories of Milwaukee and Schenectady and the hard-scrabble tenant farms of Oklahoma, it should also evoke the mines and working-class neighborhoods of Butte. That city, Jerry W. Calvert argues, was a center of the reform and radicalism that sought to challenge and remake industrial America in the early years of this century. Butte's experience, like those of its better-

known contemporaries, was a barometer of the rise and fall of the old Socialist party.

Butte was built on copper, and mining entrepreneurs set the pattern of its economic development. So did the growing corporate concentration within the copper mining industry. By the turn of the century, a single firm—the Anaconda Copper Mining Company—carried four-fifths of the city's miners and smelters on its payroll and exerted great power and influence as Montana's biggest employer. But the Anaconda's hegemony was not complete. Butte, Calvert writes, was "both a union town and a company town" (p. 4), a bastion of organized labor where a workingmen's union had been founded as early as 1878, where the closed shop was commonplace, and where the militant Western Federation of Miners was born. The strong local labor movement pursued its interests through politics as well. Butte had a Socialist mayor from 1911 to 1914, and Calvert concentrates on the electoral battles that punctuated the Progressive era in the city.

Despite his focus on Butte, Calvert's real concern is the hoary question of why socialism failed in the United States. His answers are sound but offer little that is new: vertical cleavages within the working class, party sectarianism, cooptation by reformers, trade unionists eschewing independent labor politics, and the inherent limits of socialism in one city all undercut the Left's bid for power. Much less persuasive is his assertion that the "sheer dominance of capitalist culture in the minds of many workers" also stymied radicals (p. 146). So sweeping a claim about so amorphous a thing as "capitalist culture," accompanied by neither discussion nor evidence, is inexcusably glib.

The strengths and weaknesses of the book have much to do with Calvert's perspective as a political scientist. He tabulates election returns in great detail and finds the Socialists' strongest appeal in Butte's working-class wards. That is certainly important. Elections are significant indexes of the play of power in a community, as indeed are the strikes Calvert describes. Yet they are not the only ones. Informal workplace struggle is an equally important sign of social tension, but he includes none of it. Despite the centrality of copper mining to Butte's class conflict, we learn precious little about the job culture of copper miners—something that surely affected their behavior in and out of the polling place. Calvert's chapter, "Class and Community," likewise suffers because of his penchant for relying on numbers for explanations. The figures correlating ethnicity, occupation, and voting that he uses to render the city's social profile are valuable, but the profile seems a bit schematic and flat. Apart from acknowledging the conservative influence of the Catholic church and reproducing some splendid photographs, the author leaves much of Butte's social texture unexplored, and his preoccupation with the ballot leads him to overlook promising avenues of inquiry. In recounting the crucial election of 1915 that the Socialists lost, for example, he points out that the electorate had nearly doubled since the last poll, and he attributes

the higher turnout to the recent enfranchisement of women (pp. 99–100). Those new women voters rejected the Socialists, he concludes, but that begs the question why women per se did so, because the party evidently held on to its traditional working-class supporters. If the Socialists did not pick up a commensurate bloc of new working-class women voters, then working-class women may not have been voting in numbers anything like their middle-class counterparts. That is a question with broad implications that Calvert might have pursued with profit.

But his electoral emphasis has occasional rewards. Reviewing the weaknesses of American socialism, Calvert makes what seems a narrow technical point but is probably his most important observation: the American electoral system, with its winner-take-all structure, severely handicaps any political insurgency. European social democratic parties had greater political access through parliamentary proportional representation systems. In that context, the elections that Calvert so tirelessly chronicles take on an enhanced significance.

EDWIN GABLER

Samuel Gompers Papers
University of Maryland,
College Park

STEVE GOLIN. *The Fragile Bridge: Paterson Silk Strike, 1913*. Philadelphia: Temple University Press. 1988. Pp. ix, 305. \$34.95.

The strike against the silk industry of Paterson, New Jersey, in 1913 is well known to students of labor history, especially for the participation of the Industrial Workers of the World (IWW) and for the dramatic pageant produced by the strikers. This new history by Steve Golin, however, sets the strike in a larger context and seeks to correct some earlier interpretations by historians. Golin's narrative work both places the events in Paterson in their cultural and intellectual environment and explores their social history. The essence of the author's thesis is that, in Paterson, politics and art came together in a unique collaboration between workers and intellectuals, which was of great potential in the history of the American Left.

The author discusses the different nationalities—Italians, Jews, and English—and occupational groups—ribbon weavers, dyers' helpers, and broad silk-weavers—involved in the local silk industry. In contrast to other interpretations, Golin emphasizes the unity and discipline the strikers mustered in the face of extensive community opposition and interference by the American Federation of Labor. Local 152 of the IWW, he maintains, constructed a creative strike in which the local leadership was overshadowed only after some months by supportive national figures, such as Elizabeth Gurley Flynn and William D. ("Big Bill") Haywood. The silk strikers, the author argues, were remarkable in the nonviolence that marked their militance. Borrowing tactics from the successful strike of

1912 in Lawrence, Massachusetts, and then transcending them, they collaborated with Greenwich Village luminaries in designing a pageant performed in Madison Square Garden. The pageant was, Golin insists, a successful device that won favorable publicity for the strike and further radicalized the strikers, the intellectuals, and the audience. Nevertheless, the strikers were outlasted by the manufacturers, and the recriminations following the strike's defeat were significant not only in the decline of the IWW but also as a turning point for the American Left, which thereafter could not see any hope for meaningful change in the United States. Thus, the author tries without quite succeeding to endow Paterson with broader importance than have others such as Melvyn Dubofsky, Joseph R. Conlin, Philip S. Foner, and James D. Osborne.

Golin's exploration of the alliance between the Greenwich Village intellectuals and the strikers is the richest aspect of the book. The author presents the intellectuals not as self-indulgent but rather as experiencing an enhanced class-consciousness as theories began to be worked out in practice. He demonstrates how the Paterson episode had a major impact on the views of John Reed, Hutchins Hapgood, Margaret Sanger, and others. The book also offers an interesting analysis of the major role of women strikers and middle-class feminists and explores how feminism and industrial unionism intersected. The work thus adds to our understanding, even though its conclusion is too ambitious and is overly long. Throughout, some additional editing to avoid repetition and strained metaphors would have been useful.

SALLY M. MILLER
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PHILIP S. FONER. *History of the Labor Movement in the United States. Volume 8, Postwar Struggles, 1918-1920.* New York: International Publishers. 1988. Pp. xii, 305. Cloth \$19.00, paper \$8.95.

"Rarely has labor in the United States exploded with such fury," notes Philip S. Foner, "as in the aftermath of the Great War" (p. xi). This book examines the outstanding events that shaped industrial relations in the years immediately following the end of World War I. Foner bases his interpretation on extensive research into both primary sources and secondary studies.

Never before had the nation witnessed labor strife of such magnitude. A major cause of conflict stemmed from labor's determination to preserve and enhance the gains it had won during the war. Management, however, had launched a vigorous counterattack to reverse labor's previous advances; its ultimate aim was to establish the "open" or, in reality, the nonunion shop throughout America. In addition, both federal and state authorities used the anti-Bolshevik Red Scare hysteria of this period to attack and all but destroy the Industrial Workers of the World and other radical

organizations. That intensive discord led to the Seattle general strike, the Winnipeg general strike, the Boston telephone and police strikes, a nationwide coal miners' strike, the steel strike of 1919, street railway strikes in several important cities, and strikes in the textile and clothing industries. Historians David Brody, Arnon Gutfield, and Melvyn Dubofsky have claimed that combined corporate and state power proved too powerful for labor during this era, but Foner insists that several strikes demonstrated organized labor's effectiveness. Yet, with the possible exception of victories in the textile and clothing industries, labor actually sustained a series of unrelieved defeats that provide scant substantiation for Foner's thesis.

When analyzing labor and leftist politics in this era, Foner again writes in an optimistic vein. He believes that the Russian revolution and the success of the British Labour party inspired a movement toward formation of a permanent American labor party. Yet, in fact, left-wing politics in postwar America tell a far sadder and different story. Rent by bitter internal dissent, the American Socialist party, which had captured 6 percent of the presidential vote in 1912, lost two-thirds of its supporters and by 1919 could boast of only a little over twenty thousand members. Others on the Left divided their loyalties among the Communist Labor party, the Communist party, the Committee of Forty-Eight, and the Non-Partisan League. When some tried to coalesce behind the Farmer-Labor party in the presidential election of 1920, the results proved highly disappointing. No socialist-labor party has ever emerged as a major force in American politics. Foner remains critical of Samuel Gompers's nonpartisan political approach, but the American Federation of Labor, despite its limitations, endured and helped to put food on the table of some American workers. Splinter parties on the Left had nothing to offer but pie in the sky.

Historians may disagree with Foner's interpretations, but his thoroughgoing research, factual accuracy, and clear narrative style combine to make this a most worthwhile contribution to American labor history.

GRAHAM ADAMS, JR.
Mount Allison University

SCOTT G. MCNALL. *The Road to Rebellion: Class Formation and Kansas Populism, 1865-1900.* Chicago: University of Chicago Press. 1988. Pp. xviii, 354. Cloth \$49.95, paper \$19.95.

In this book Scott G. McNall brings a wide range of methods and Marxist theories to bear on a movement that has attracted the interests of many radical scholars during the past decade. Because of his broad approach and effective use of sources, however, his observations will be of use to both Marxist and non-Marxist historians and social scientists.

A major concern of those interested in the history of Populism has been to gain an understanding of the

origins and development of the movement. McNall helps clarify these problems by presenting two models that have been used to explain Populism and then subjecting the second to rigorous analysis. The first is the liberal model of John D. Hicks that pictures social and political movements as rising spontaneously when groups of people are faced with socioeconomic grievances. The second is the currently popular social science model of E. P. Thompson that sees movements as requiring a more complex formative process. Lawrence Goodwyn, in *The Populist Moment* (1978), expressed this view most explicitly when he argued that a grievance must be followed by a particular sequence of events. These events are the creation of a protest organization; then the creation of an instrument for recruitment of members; then the creation of, and education in, a movement ideology; then action for change. Goodwyn and many sociologists label such a process as movement formation. McNall raises the level of generalization and sees the process as applying to class formation. To do this, he uses Thompson's distinction of a class "in itself," one existing as an economic entity, and a class "for itself," a self-conscious group acting as a rational agent to promote its goals. McNall thinks that crossing the threshold between the two categories is important, and he presents a complex scenario to show that the Kansas Populists failed to reach the Marxist goal of becoming a class "for itself," one capable of molding a future distinctly different from the liberal capitalist society now dominant in the United States.

The reasons that the Populists did not achieve what McNall perceives as their goals lay in their inability to meet the conditions required in the social science model as expressed by Goodwyn for a successful social movement. The Kansas farmers met the first criterion. They certainly had a grievance. Railroads, monopolies, the monetary system, and politicians worked against their interests. But, after that, McNall shows that everything required of them in the model went astray. Stages one and two, movement organization and recruitment, did not occur as needed. Alliance lecturers organized at a whirlwind pace in late 1888 and 1889. They recruited anyone, played down discussion and dissent, and produced a heterogeneous organization that was frequently at war with itself. Contrary to Goodwyn's contention that cooperatives played a key role in any phase of the movement, the education phase of movement development similarly fizzled. Since the Alliance members had no cooperatives to teach them the nature of their enemies, they received their education only when they formally entered politics in 1890. At that time the organizational process was given its final shock. The movement was betrayed by its leaders. Most of the Kansas leaders were professional reformers with few ties to the real farm community. Because of their political orientation, they opted for abandonment of the farmers' economic interests and led in the movement for fusion with the Democrats. With little creative leadership, a quickly formed and fragile grass-roots organization, no commonly devel-

oped ideology, and little sense of community, the movement faltered. After 1896 the farmers in the few remaining suballiances acted rationally and abandoned the Populists for the now more flexible Republicans. They had failed, through their own mistakes, to become a class "for itself."

McNall has produced a useful book. His theoretical orientation and use of social science methods create a very structured and comprehensive study of Populism at the state level. A non-Marxist can substitute "effective interest group" for "class for itself" and feel reasonably comfortable with the work. But the larger question is the validity of the sequential movement culture model as applied to Populism in Kansas. When McNall denies that Kansas farmers became a class "for itself," he is using exactly the same sequentially ordered criteria as others use when they proclaim Populism a social movement. McNall presents impressive evidence to support his case that no such sequence of events occurred in Kansas.

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ELLIOTT SHORE. *Talkin' Socialism: J. A. Wayland and the Role of the Press in American Radicalism, 1890-1912*. Lawrence: University Press of Kansas. 1988. Pp. xi, 280. \$25.00.

The weekly newspaper *Appeal to Reason* has a firm if unusual place both in American radical history and in the story of American journalism. For during its heyday, the first dozen years of this century, the *Appeal to Reason* (with its broad-based circulation in the hundreds of thousands and an attractive, successful, evangelical Left stance) stood at the "intersection of Socialist agitation and American culture" (p. 4). The weekly did so in a way and a manner that really have not been rivaled since. This unusual connection has not escaped the attention of other scholars, most notably Paul Buhle ("The *Appeal to Reason* New Appeal," in Joseph Conlin, ed., *The American Political Press*, vol. 1 [1974], pp. 50-59).

Elliott Shore has produced a striking and well-written book and has generously acknowledged the efforts of his predecessors. But he has done much more than just build on their efforts. In firm command of recently discovered archival material, he has provided a wealth of new insights and a plethora of detail in chronicling not merely the rise and fall of the *Appeal to Reason* but also the dramatic life of its founder and guiding light, Julius A. Wayland. After Wayland's suicide in 1912, the weekly carried on for another decade under various editors with differing editorial stances, but the *Appeal to Reason* was never quite the same. After 1912 the weekly "ceased to be a radical publication in the sense that it no longer was a rallying point for a movement; rather it was an outlet for publishing stories that were not

politically appropriate for mainstream papers" (p. 221).

With great sensitivity and balanced judgment, Shore has carefully delved into Wayland's career, one that is very American for its time, being grounded in mid-western radicalism and the late nineteenth-century American booster spirit. Wayland, for all his commitment to the socialist cause, apparently never formally joined any socialist party and shied away from the structural Marxism practiced by many of his peers in the East. Before starting the *Appeal to Reason* in 1895, he led an eclectic life, including successful stints as "a tough businessman, a Republican editor, a Populist, and a real estate speculator" (p. 91). He overcame the weekly's initial financial problems by making dynamic use of modern selling techniques to build up circulation and "non-movement" advertising. Wayland and his associates established a minor publishing empire (of considerable importance at one point to American radicalism), which at its height included numerous regional editions, dozens of pamphlets, many serious book titles, and other socialist-oriented periodicals.

An expert in the history of the alternative press, Shore makes clear Wayland's editorial philosophy (and the rationale behind it), which proved so effective in the weekly's columns. The *Appeal to Reason* in its heyday, with articles such as the serialization of Upton Sinclair's *The Jungle*, emphasized reform as well as radical change. *Appeal to Reason* and the other publications in Wayland's stable did not blindly follow any party line. Shore serves as a marvelous cicerone in guiding the reader through the tangled internecine strife that marked this period of American socialism and that threatened Wayland's efforts. And Shore does not fail to deal with the obvious contradictions inherent in any enterprise that uses the most up-to-date entrepreneurial capitalist techniques to publish a newspaper dedicated to radical change. Shore displays considerable perception in handling this intriguing subject.

It is possible to quibble with some of Shore's conclusions about American socialism, to argue that he has failed to set Wayland and his ventures properly in the context of modern periodical publishing, to point out that Shore could have done more with the editorial content of the *Appeal to Reason* (especially letters from readers, all of which were published and which sometimes took up as much as 25 percent of the weekly), and to regret that the author did not sufficiently integrate the story of the *Appeal to Reason* with the history of the American radical press. But overall these are minor caveats and should not detract from Shore's accomplishment. He has written a fine worthwhile book that may help readers formulate an answer to Werner Sombart's oft-repeated question, "Why is there no socialism in America?" Finally, it should be noted that Shore's work benefits from an attractive array of photographs, carefully chosen and usefully scattered throughout the text.

DANIEL J. LEAB
Seton Hall University

RICHARD E. WELCH, JR. *The Presidencies of Grover Cleveland*. (American Presidency Series.) Lawrence: University Press of Kansas. 1988. Pp. xi, 268. \$25.00.

Thanks largely to Allan Nevins's laudatory 1932 biography, historians once tended to rank Grover Cleveland considerably above his contemporaries in the late nineteenth-century presidency. But since the 1950s, when Horace Samuel Merrill cast Cleveland as a "Bourbon Democrat," many scholars have been less admiring, accepting Merrill's view of Cleveland as a compliant confederate of large business and financial interests. Richard E. Welch, Jr., rejects both the mythology of Nevins and the demonology of Merrill in this excellent study. Welch presents Cleveland as a hardworking politician and chief executive who was committed to doing the best job he could but who ultimately failed in his second term for a variety of reasons, many traceable to Cleveland himself.

Welch's book is doubly valuable: first, to specialists it offers new insights into Cleveland's leadership; and, second, to students it recounts in brief compass the central features of his administrations. Welch devotes a chapter to several issues that changed little between the two terms: civil service policy, veterans affairs, race relations, and public land policy. He treats more extensively major questions, such as the currency issue, the tariff, and conflict between labor and management, which altered with changing circumstances during the two terms but toward which Cleveland maintained essentially consistent attitudes. Welch amply covers the domestic concerns that dominated these years, but as a diplomatic historian he is also quite at home in the realm of foreign affairs and accords the controversy with Great Britain over the Venezuela-British Guiana boundary the most extensive coverage of any single episode in the book.

Welch's central concern is to delineate the nature of Cleveland's political personality. Acknowledging his debt to other writers, including Nevins and Merrill and, more recently, Robert Kelley and Geoffrey Blodgett, Welch occasionally incorporates but more often transcends their views to offer a fresh analysis of Cleveland's political leadership. The key to understanding the "contradictions and ambiguities of the Cleveland presidencies," he asserts, is to recognize the "continuing tension between Cleveland's inherited political faith and his assertive, aggressive personality" (p. 8).

Although Cleveland never articulated an elaborate statement of his political philosophy, he tended to accept the wisdom of classical liberalism's belief in limited government interference with immutable economic laws and the Whiggish ideal of the chief executive as primarily an administrator who left policy formulation to the legislative branch. Yet, in practice, says Welch, Cleveland's assertive, often-combative personality drove him to actions that contradicted these basic tenets. Taking Andrew Jackson as his model, Cleveland saw himself as the tribune of all the people. Like the

Old Hero, he could at one and the same time denounce the centralization of power at the federal level of government and yet work to augment the power of the national executive as the chief bulwark of the interests of the people. Not surprisingly, Cleveland's approach was more defensive than creative. In working for his aims, he tended to shun negotiation with antagonists and relied instead on exhortation to duty as he saw it. Ultimately, his assertiveness and self-righteousness proved his undoing as a leader. More inclined to condemn than to conciliate his opponents in Congress, he exhibited in the last two years of his presidency a solipsistic rigidity that operated not to extend his influence but to destroy it.

So brief a recapitulation does not do justice to Welch's thoughtful and sensitive treatment, relayed in his characteristically clear and graceful prose. One should add that one of the great virtues of this book is that it reminds American historians that they must abandon their tendency to neglect national politics in the late nineteenth century if they would understand the transformation of the American political universe from the Civil War party system, dominated by section and race, to the political system of the twentieth century, when economic and social concerns and foreign policy matters came to occupy center stage.

CHARLES W. CALHOUN
East Carolina University

JOHN ENSOR HARR AND PETER J. JOHNSON. *The Rockefeller Century*. New York: Charles Scribner's Sons. 1988. Pp. xviii, 621. \$29.95.

John Ensor Harr and Peter J. Johnson's book is a sprawling and oddly titled work whose focus is the philanthropic activities of John D. Rockefeller, John D. Rockefeller, Jr., and John D. Rockefeller III between 1889 and 1952. (A projected second volume will cover the last quarter century of John D. Rockefeller III's life.) The authors, both formerly associated with his operations, benefited from access to the family archives in Pocantico Hills as well as from personal and foundation papers closed to other researchers. Their "warts and all" (p. xv) coverage treats the family sympathetically and touches, sometimes briefly, on a range of topics, including the Rockefeller Foundation, Rockefeller Center, the post-World War II peace treaty with Japan, the Museum of Modern Art, Colonial Williamsburg, the Institute for Pacific Relations, and Arcadia National Park.

Most of the key historical elements of Rockefeller philanthropy are outlined in the first third of the book. By the 1890s the volume of requests and donations swamped John D. Rockefeller's personal, meticulous approach to giving, which was based on deeply held values of piety and stewardship. Building on systematic business principles, Rockefeller hired Frederick Gates to advise and manage his donations, which by the early 1920s totaled nearly \$500 million. At Gates's urging, he

consolidated his philanthropy in the century's early years in such organizations as the Rockefeller Institute for Medical Research, the General Education Board, and the Rockefeller Foundation and shifted from "retail" giving to individual recipients to "wholesale" donations through institutions.

Imbued with his father's sense of duty, John D. Rockefeller, Jr., made philanthropy his career and worked under Gates to emphasize funding for causes and cures rather than for the alleviation of symptoms and to apply scientific research, expertise, and careful follow-up. As Rockefeller, Sr.'s and Gates's roles faded after 1917, John D. Rockefeller, Jr., controlled the family philanthropy in the 1920s as chairman of the Rockefeller Foundation and as the source of millions of dollars of personal donations for conservation, historic preservation, and other interests. Subsequent portions of the book detail the reduction of his largess after the depression of the 1930s and the increasing role of his sons in directing Rockefeller philanthropy by the early 1950s.

Although the presentation clearly demonstrates the impressive scale and scope of family contributions, it does not support the authors' claim that John D. Rockefeller, Jr., was "one of the great figures in two centuries of American history" (p. 557). Their portrait of him, which relies heavily on secondary works and which contains scant direct evidence of his motives, gives him only third place to his father (whose role is treated quite briefly) and to Gates before 1917. Even after he assumed the dominant position he held until mid-century, his contributions toward policies, strategies, and ideas remain vague and abstract. The innovators and actors are experts such as Abraham Flexner and professionals such as Wallace Buttrick and Raymond Fosdick.

A more serious flaw is the authors' loss of control in the book's last half as they follow a diffuse chronological narration. There are useful insights about the evolution of the Rockefeller Foundation, the development of Rockefeller Center, and the transition of power within the family. Also quite clear is the decline of private giving in America's richest and most generous philanthropic family in the face of the Great Depression, changing tax law, and the division of family wealth among six children. But the discussion is presented in fragmented fashion and is padded with detailed description of doubtful relevance. Coverage of John D. Rockefeller, Jr.'s other five children appears in superficial summaries spread across the narrative.

Treatment of John D. Rockefeller III, which depends heavily on his diaries, is much more detailed and at times frustrating. His formal, deferential relationship with his stiff, remote, and domineering father comes across vividly. He accounted to his father for his income until his twenty-fourth year and communicated with him in lengthy, punctilious letters about disagreements. In his efforts to establish a career, he followed his father's footsteps but with conflict and little independence in such cases as succession at the Rockefeller

Foundation, management of the family estates at Pocantico Hills, and shared power in Colonial Williamsburg, his father's favorite project. None of the extensive data on family relations and child rearing is informed by theory or framework.

More troublesome is the unanswered question of why we should read a lengthy biography of John D. Rockefeller III's first forty-six years. By the authors' own admission, he was "a late bloomer" (p. 551), who until 1952 was a marginal, self-effacing character. His role at the Rockefeller Foundation was resented and restricted by key trustees, and he was overshadowed by his younger, more energetic, and more successful brothers, Nelson and Laurance. One wonders if we would read any of this if the man had not been named Rockefeller. Certainly readers would benefit from a briefer, more focused account.

CHARLES CHEAPE
Loyola College

JON H. ROBERTS. *Darwinism and the Divine in America: Protestant Intellectuals and Organic Evolution, 1859–1900*. (History of American Thought and Culture.) Madison: University of Wisconsin Press. 1988. Pp. xviii, 339. \$26.75.

In the four decades following the publication of the *Origin of Species*, the reaction of Protestant intellectuals developed in two distinct if overlapping phases, Jon H. Roberts argues in this extensively researched study. Having fashioned alliances with "science" as they understood it, Protestant thinkers essentially ignored the *Origin* prior to 1875, less concerned that it was anti-Christian than that it was unscientific. After the mid-1870s, as the number of anti-Darwinian scientists dwindled, the emphasis shifted. Critics of Darwinism, now a minority among Protestant intellectuals, sought to invalidate the theory by demonstrating its inconsistency with the Christian message. Meanwhile, a larger number of Christian evolutionists argued that it was possible to accept Darwinism without abandoning the faith. Although winning that battle, they ultimately lost the war. By making "scientific" matters a monopoly of trained practitioners, they contributed to the process whereby "the locus of cultural authority" for educated Americans shifted from theology to science (p. 241). Religion, if not opposed to science, became a good deal less interesting.

Making this case, Roberts alternately confirms and qualifies the conclusions of a number of related and remarkably good studies that have appeared during the past decade. Nineteenth-century portraits of the "warfare" between science and religion, it is now safe to say, were flawed insofar as they pitted "clergy" against "scientists," as Roberts again demonstrates for the period prior to 1875. Joining Neal C. Gillespie, Frank M. Turner, and others, Roberts nonetheless believes that the battle for cultural authority was real and the stakes high, the Protestants' fatal mistake being their

deference to the scientific community. Although his Christian evolutionists resemble the Christian "Darwinisticists" of J. R. Moore's *The Post-Darwinian Controversies* (1979), Roberts denies that Herbert Spencer played a role in shaping their world view. Whatever their faults, these accommodationists were finally Christians, while Spencer and many other evolutionists were not. Again challenging Moore's work and also George Marsden's *Fundamentalism and American Culture* (1980), Roberts insists that Protestant willingness to embrace evolution was less the result of a "fit" between its philosophical assumptions and transmutation theory than the product of its conviction that speciation was an issue to be judged by scientists.

Awarded the Brewer prize of the American Society of Church History, this book provides the most detailed account to date of the reaction of American theologians to Darwinism. Yet, despite Roberts's exhaustive research (the notes fill eighty-seven pages), his work remains suggestive rather than conclusive. Some issues are left unresolved, for example, the role of natural theology in the development of Darwin's theory and the question of why Darwinism presented a unique and severe challenge to thinkers already skilled in reconciling religion and science. Other issues are simply not addressed. Given Roberts's emphasis on a struggle for cultural authority between two intellectual communities, one yearns for a survey of the structure of the Protestant intelligentsia. Instead, countless references to a "few," "many," "a number," "not all," "a sizeable minority," and so forth, suggest that Roberts thinks that numbers rather than social influence or other sociological factors are, after all, the critical issue. Viewing the struggle for "cultural authority" as something apart from ideas themselves, he also treats "Protestant intellectuals" as a monolith, ignoring entirely the role of denominational affiliation in shaping the response to Darwinism. Moreover, in challenging Moore, Marsden, and others, whose work appeared essentially after this one was in its doctoral stage, Roberts tends to state his differences in notes (for example, "I think," "my reading of the sources has convinced me") rather than to demonstrate the case with references to the evidence. In treating Asa Gray, for example, he addresses the issue of Calvinism and Darwinism briefly and inconclusively in a note (p. 294 n. 42), while ignoring it in the text itself (pp. 18–20, 38–40).

These criticisms aside, this study is richly suggestive and a gold mine of information. Roberts deserves praise for a first book of such depth and complexity.

ROBERT C. BANNISTER
Swarthmore College

ROBERT V. BRUCE. *The Launching of Modern American Science, 1846–1876*. (The Impact of the Civil War.) New York: Alfred A. Knopf. 1987. Pp. x, 446. \$30.00.

This is a book whose subject is important to the academic specialist and to the elusive general reader.

Robert V. Bruce's compass is the professionalization of science from the 1830s through the turn of the century, when its subjects "were fully formed and rising to their cloud-wrapped destiny" (p. 3). To support this claim the book correctly focuses on the "process . . . the internal sociology, economics, and politics of science and on its interaction with the larger society" (p. 6).

The core of Bruce's volume traces the creation of national institutions that provided houses of the intellect in which a new class of professionals could flourish. Government and state geological surveys, new and reformed universities, and processes of scientific communication all matured parallel with the disappearance of amateurism. Interwoven are stories of scientific controversies, personal animosities, and the congruent rise and fall of science establishments. Chief among the key players on the stage of premodernism are men who are mainly quiet, self-effacing, rational, and orderly. Among the heroes of this tale are the botanist Asa Gray, the geologist James Dwight Dana, the chemist Oliver Wolcott Gibbs, and the physical scientists Joseph Henry and Alexander Dallas Bache. Emboldened by the spirit of the "American eagle . . . full-fledged and screaming, in cultural and intellectual matters," such men and the institutions they captained produced a scientific culture of notable attainment that handsomely rewarded "Baconian" data gathering and accommodated elitisms shaped to accord with national values.

Bruce's absorption with detail, inclusiveness, and trivia stifles the process of imaginative intermix between reader and writer that constitutes literary creativity. The author largely abjures conceptualization and selectivity. Nothing, absolutely nothing, is left to the imagination. Block after block, stone after stone, plank after plank, Bruce builds an edifice from every piece of available material. At last, the undesigned building is finished—a massive Victorian house, with every window shuttered, layered with gingerbread. The reader is wearied by repetitive monotone, a condition heightened by writing consistently marred by parenthetical clauses, compound verbs, odd colloquialisms, and bad English usage.

The reader has to suffer through passages such as those affirming that scientists who lived in urban settings "welcomed the electricity generated by the rubbing together of minds," a process that took place largely in the Northeast where Boston and Cambridge were twins of "a binary star" (p. 30). Authorial asides and the doings and "roving" of a host of minor characters continually get in the way of the large story. In the important area of natural history collecting, for example, the zoologist Spencer F. Baird "made use of Americans abroad, including the footloose author of *Home Sweet Home*, and science-dabbling Army officers" (p. 66). Physiological determinism enters the picture in the case of the geologist William Barton Rogers who left Virginia because of the intellectually barren environment, not because he had the face to do it: "though William had the lean, craggy Yankee face of an Emer-

son, nothing in his ancestry and upbringing turned him northward" (p. 60). This observation reflects a larger, imbalanced, and condescending view of antebellum southern science, whose votaries are described as preferring "less physically arduous fields" than natural history. "Meteorology . . . had many nonslave data gatherers. The weather came to their verandahs and called for nothing more than checking of thermometers, barometers, and rain gauges." The "field of science in which the South almost approached its population quota was mathematics. Of all science, . . . freest of physical effort" (p. 63).

Such observations underscore another distracting technique of the author, that is, his compulsion to attempt to quantify many judgments about American scientists and science by the standard of "leading" savants whose lives and careers he has analyzed with great thoroughness through years of studying the *Dictionary of American Biography*. The material should have been neatly summarized in the text, and the essential remainder placed in an appendix. Instead, intrusive quantification serves only to confuse and sidetrack the reader, and its results sometimes bemuse the author, leading him to offer vague explanations for cultural, regional, and individual distinctiveness.

The volume also presents some questionable judgments and explanations. It is never really clear what the "impact" and meaning of the Civil War was for science, and the transformation of the war is the special subject of the series in which the book appears. With all of its details, the volume really does not define or specify what is peculiarly "American" about the science or social processes described, aside from a vague environmental determinism that encouraged data gathering. Louis Agassiz is used somewhat unfairly both as a hero to introduce the scientific scene in 1846 and as a hounded, tragic figure whose downfall from grace highlights the achievements of those at odds with him personally and intellectually. In view of the past accomplishments of the author, and the labor of more than twenty years so tirelessly poured into this book, it is unfortunate that Bruce did not realize that less can really be more. All of the data in the book cries out for the author to rise above his material and conceptualize such issues as the typology of establishments and their special relationship to national culture. Aided by a skilled editorial hand, his real contributions would have shown through far more cogently. Those, incidentally, did light the eyes of the Pulitzer Prize committee members, who awarded this volume the history prize in 1987.

EDWARD LURIE

University of Delaware

F. G. GOSLING. *Before Freud: Neurasthenia and the American Medical Community, 1870–1910*. Urbana: University of Illinois Press. 1987. Pp. xviii, 192. \$22.95.

Neurasthenia was a psychiatric diagnosis popular among American physicians in the United States for about forty years. It was used to categorize a wide range of behaviors that today we call neuroses and stress disorders. The word "neurasthenia" literally referred to a weakness of the "nerve force," which was manifested in both psychological and physical symptoms. To many late nineteenth- and early twentieth-century doctors, it was the archetypical "American disease," produced by the stresses of urbanization, industrialization, and competition.

Because its pathological basis was so difficult to locate (although its proponents were convinced that one existed), F. G. Gosling argues convincingly that neurasthenia provided an exceptionally useful vehicle for the expression of the cultural values and preconceptions of the physicians who employed it. Class and gender biases were particularly obvious in explanations of its causes. For example, nervousness in businessmen most often was attributed to overwork; in laborers, to alcohol or drug abuse; in women, to biological weaknesses, most often of the reproductive system. (In contrast, other popular late nineteenth-century diagnostic categories such as acute mania and subacute dementia seemed tied fairly clearly to behavioral cues and to evoke more consistent therapeutic responses from neurologists and asylum superintendents.) According to the leaders of the American Neurological Association, neurasthenia was most common among the "brain-working class of the major cities" and their female partners (p. 83). Convinced of the greater toughness of the nervous system of lower-class Americans, physicians encountering high-strung domestic workers or laborers in dispensaries were more likely to diagnose them as dyspeptic or hypochondriacal than as neurasthenic. Although Gosling does not say so, I also suspect that physicians more readily perceived danger in overwrought members of the working class and thus more often diagnosed them in such a way as to justify institutional care. How and when physicians drew the line between neurasthenia (not considered a form of insanity) and full-blown melancholia is not discussed by Gosling and would be an interesting question to pursue.

Gosling is not the first historian to look at the sociomedical bases of neurasthenia. As he readily admits, he follows in the path of Barbara Sicherman, Bonnie Blustein, Anita Fellman, Michael Fellman, and S. P. Fullinwider, among others. Yet his analysis of the cases of 307 neurasthenic patients described in medical journals between 1870 and 1910, when combined with an examination of more general journal articles on the topic and a collective biography of their authors, yields new insights into the ways in which neurasthenia was conceptualized and treated. First, he claims that neurologists, rather than general practitioners or non-neurological specialists, dominated the discussion of neurasthenia in medical journals. Second, he finds a surprising occupational diversity among the patients described in the medical literature (given neurologists'

association of neurasthenia with the upper classes); for example, 56 percent of them were skilled or semiskilled laborers. In comparing the cases of male and female neurasthenics (whose numbers were almost the same), Gosling finds sharply sex-differentiated explanations and treatments for the same symptoms. Because women had a "natural" tendency toward nervousness, their complaints were taken less seriously. Notions of psychology based on gender also benefited women, however, in that they were more often "cured" by neurologists, in part because some nervousness was considered acceptable in female patients. It would be interesting to juxtapose some of the firsthand accounts of neurasthenia produced during this period with the doctors' descriptions so deftly analyzed by Gosling. Gosling's discussion of the treatment of neurasthenia in the early twentieth century makes clear the inadequacy of histories of modern American psychiatry that begin with Freud. In particular, he calls for more credit to be given to the work of George Beard. Gosling claims that, despite his flaws, Beard made two significant contributions to American psychiatry. He "legitimized functional emotional imbalances by giving them a non-derogatory label" (p. 137), and he pioneered in the medical treatment of such problems. Finally, on the basis of the broad range of physical and psychosomatic symptoms reported in the case histories, Gosling concludes that yesterday's neurasthenia is today's stress. And he argues, again in defense of late nineteenth-century neurology, that our understanding of the "pathology of stress" (p. 33) has advanced little in the past seventy to eighty years.

This book expands our understanding of an important process: the impact of social, professional, and economic issues on doctors' responses to both physical and psychological symptoms. It also is easily accessible to a broad audience of both historians and mental health workers. If Gosling continues his present line of research, I would like to see him extend his concluding remarks on the process whereby, with the introduction of Freudian psychology into the United States, psychiatrists appropriated the clients (and therapeutics) of late nineteenth-century neurology and neurologists turned to the management of nerve diseases (much more narrowly defined) and of brain injuries.

ELLEN DWYER
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Bloomington

WHITFIELD J. BELL, JR. *The College of Physicians of Philadelphia: A Bicentennial History*. Canton, Mass.: Science History. 1987. Pp. viii, 326. \$40.00.

Created in the same city at the same time as the U.S. Constitution, the College of Physicians of Philadelphia celebrated its bicentennial in 1987. This volume falls squarely into the genre of commemorative chronicles but avoids the inclusion of mere lists of fellows and officers. No doubt production costs demanded respect

here; from twenty-three senior and junior fellows in the college in 1787, the number of members has risen to over two thousand. This book is, after all, a history of a medical institution, not a prosopographical analysis of its membership.

Whitfield J. Bell, Jr., did not set about to portray the college as a perennial pacesetter in advancing medical research or reform or as the home of a world-class library. These things it has not been for the most part until well into the present century. Bell gives us a history of the college warts and all, and this approach surely produces one of the important contributions of the book. It is conflict and not consensus that flavors the first one hundred thirty years of its existence. Squabbles among fellows over epidemics and therapeutics often resulted in Philadelphia trailing rival Boston in the production of informed speculations. Long after Benjamin Rush drew opposition from fellows for advocating (in 1793, the first year the *Transactions* were published) a ten-gram dose of calomel and the removal of ten ounces of blood from patients suffering from yellow fever, fellows of the college were often contentious and, on occasion, contemptuous of one another.

The affinities of traditional institutional history for describing the details of funding such endeavors are tempered here with useful examples of how one institution mirrors activity elsewhere. During the nineteenth century, members of the college split over such issues as the contagiousness of cholera and puerperal fever, whether anesthesia should be administered to women in childbirth, and the use of the insanity plea in murder cases. The college often comes out with a little mud on its institutional nose, and "great men" play their roles. The contributions of an S. Weir Mitchell were at times no match for those made by gynecologist Charles D. Meigs (who did not believe, as did Boston's Oliver Wendell Holmes, that puerperal fever was contagious or that women in childbirth ought to have anesthesia to reduce Eve's original pain). But in 1886 the college did help defeat an antivivisectionist bill that would have inhibited research. In the 1930s, fellows spoke against national health insurance but agreed to the election of women to the college. Bell also reminds us of contributions to the study of medical history in the United States made by fellows Francis A. Packard, George W. Norris, and Edward B. Krumbhaar, who in 1924-25 helped organize the American Association of the History of Medicine (AAHM). The AAHM soon began publishing what today remains a major journal in the field, the *Bulletin of the History of Medicine*, with its permanent editorial offices at the Johns Hopkins Institute of the History of Medicine.

Bell's book is based almost exclusively on college documents. One cannot fault him for not going to the sources and producing a tightly focused study. But, although he produces a recent summary of a now-venerable medical institution, recent scholarly monographs on related subjects within a larger geographical context are notable by their virtual absence in the notes (there is no list of references). Although the college

never sought the power to regulate practice, it did aim to foster professional pride, scientific enterprise, and public spirit among the city's healers. The college has maintained purity of institutional intent while always in process and always underfunded. The narrative itself concludes in 1969, yet Bell provides a necessary base for asking related questions raised by Edward C. Atwater ("Medical Schools: How Should We Write Their Histories?" *Bulletin of the History of Medicine* 54 [1980]: 455-60). Given Atwater's guidelines, future studies should seek to determine the nature and significance of overlapping membership or affiliation between college fellows and other Philadelphia medical institutions and take a closer look at the composition of the college's membership itself over time.

ERIC H. CHRISTIANSON
University of Kentucky

MARTHA H. VERBRUGGE. *Able-Bodied Womanhood: Personal Health and Social Change in Nineteenth-Century Boston*. New York: Oxford University Press. 1988. Pp. viii, 297. \$29.95.

Martha H. Verbrugge seeks to add a new dimension to the study of women's health issues in nineteenth-century America by tracing the health reform movement in Boston from 1830 to 1900. Specifically, she deals with three institutions: the Ladies' Physiological Institute, Wellesley College, and the Boston Normal School of Gymnastics. Although the book focuses on institutional history, Verbrugge's study combines social and intellectual history and incorporates local history, prescriptive literature, and, occasionally, individual commentary. The author states as her larger goal the examination of the relationship between personal health and social change in nineteenth-century Boston.

As Verbrugge's title demonstrates, the issue of health in nineteenth-century America was not only a political and social one but also a gender issue. Able-bodied womanhood, she insists, had a cultural as well as a physical meaning. But the precise meaning remains elusive in this book because Verbrugge does not explore as deeply as she might the political, social, and psychological uses of female weakness. Although her research is meticulous, she cannot come to terms with the clear discrepancy she finds between statistics on female life expectancy and mortality and the grossly exaggerated popular and medical perception that women were greatly at risk. She acknowledges the distance between reality and perception, but she tries to deal logically and rationally with a phenomenon both illogical and pervasive in nineteenth-century American life. Why did a largely exaggerated notion of female ill health become "the symbol of and at times a scapegoat for" (pp. 20-21) the upheavals in American society in the middle decades of the nineteenth century? For the most part, this book skirts that very intriguing question.

One suspects that Verbrugge abandons the question because her institutional focus is not capable of yielding

an answer. The social creation of female invalidism needs to be explored in the context of changing definitions of masculinity and maleness. The concept of the self-made man, with its emphasis on self-control, had alarming ramifications when medical doctors applied it to woman's reproductive system. The pervasive use of a male medical model meant that doctors looked at women as mutilated males. By the mid-nineteenth century, a new understanding of the periodicity of woman's menstrual cycle led to the conclusion that she was out of control, a prisoner of sex.

What did women make of the situation? Here Verbrugge's research on the Ladies' Physiological Institute might yield rich results. Were the middle-class women who scandalized Boston society by using female mannequins to demonstrate physiological principles in rebellion against the medical establishment, much as were the women in the 1970s who bought *Our Bodies, Ourselves* and learned to use a speculum? Apparently not. Verbrugge concludes that the institute succeeded by combining sorority and physiology and that it largely avoided controversy, radicalism, and feminism.

Verbrugge is on more solid ground when she abandons the larger issue of the social uses of gender and deals with institutions and how they changed over time. Her study of physical education at Wellesley and the Boston Normal School of Gymnastics demonstrates how women pioneered the concept of able-bodied womanhood and expanded, both physically and professionally, the boundaries of women's actions.

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LINDA GORDON. *Heroes of Their Own Lives: The Politics and History of Family Violence; Boston, 1880–1960*. New York: Viking. 1988. Pp. xiv, 383. \$24.95.

That family violence is a social problem, Linda Gordon observes, is a relatively recent notion. Although family violence eventually encompassed a number of categories (sorted out by Gordon as child abuse, child neglect, incest, and wife battering), its first articulation began with the late nineteenth-century discovery of "cruelty to children," a term patterned after reformers' earlier interest in cruelty to animals. From these relatively humble beginnings grew a web of agencies that became increasingly more powerful in their ability to frame the discussion of what was considered family violence, who its victims were, and what the state should do to correct it. Gordon's book is a study not of family violence throughout the class structure of American society but of those groups who could not conceal their conflicts from others (a focus necessitated by the limited sources available). In urban Boston before 1960 clients of child-saving agencies were especially likely to be poor people: single mothers, the transient, and the unemployed, many of whom were Irish and Italian immigrants struggling to make the transition from peasant

family life to an urban wage-earning world that loosened the hold of parents and community restraints.

The connections among patriarchy, poverty, and family violence present a continuous knot of complexity in this book, and, in keeping with the feminist methodology that she uses so successfully, Gordon refuses to give us a pat formula to its untangling. But, although she offers no final resolution, she rethinks a number of categories used by historians. The family, she argues, has usually been conceptualized as a unit. That approach masks the reality that families are composed of individuals and that struggles over power, resources, and behavior take place within them. The notion of studying the family usually means seeing the father as its representative, which tends to render other family members invisible. Protests against invading the privacy of the family usually signify a desire to protect the privacy of the father. To get at ways to discuss that privacy and its potential for the abuse of power, Gordon suggests a narrower definition of patriarchal control than that currently used. Patriarchy should not be synonymous with male dominance throughout history and society (and therefore both ahistorical and universal) but rather signify "a form of male dominance in which fathers control families and families are the units of social and economic power" (p. vi).

The wives and mothers that Gordon discusses were usually dependent for their self-esteem and economic well-being on control of children, so both parents often had a vested interest in retaining patriarchal forms. But urban environments undermined the psychological and economic hold of patriarchal authority. As children struggled with parents over going to school and handing over wages, as wives tried to extract enough money from husbands to feed and shelter their children, as fathers sought to maintain traditional obedience, fights ensued. Although men almost always had the strength and economic resources to extract compliance and to inflict the greatest physical abuse, family violence was not, Gordon insists, a simple matter of individual brutality inflicted on powerless victims. It was, rather, the product of family conflict.

Gordon thinks that the stress, discouragement, and erosion of self-esteem that tend to accompany chronic poverty may certainly have exacerbated family violence but were not the primary causes of it. Most families who lived in poverty were not perceived to be practitioners of family violence. What social workers considered child abuse or neglect was often what the poor considered to be traditional discipline or conditions beyond their power to correct. Over time, however, the notion that all poor children were neglected or abused became fairly commonplace.

Gordon agrees that psychological disorders, alcoholism, and chronic depression contributed to abuse. But she presents examples, particularly of neglectful mothers, to show that persistent wife battering, and the impossibility of escaping abusive husbands and fathers, may have induced those personal disorders in the first place. The economic dependence of women on men

was what kept the troubled family together and perpetuated the worst kinds of family violence. Social workers were usually blind to this reality. Convinced that fatherless families and working mothers were the worst possible outcomes for children, they dismissed evidence of incest and wife battering in order to keep families together. By the 1940s, with psychoanalytic explanations at their peak and feminism at its nadir, these forms of family violence, in particular, were often blamed on mothers and daughters. Girls who claimed to be victims of sexually abusive fathers were, in line with popular Freudian interpretations, presumed to be either fantasizing or seductive. If men lost control and abused their wives and children, wives were accused of being domineering, unsympathetic, or frigid. Family violence was something for mothers to resolve.

The social workers who reported, intervened in, or failed to recognize the incidences of family violence in Boston receive less attention than their clients in this study, but Gordon does provide a history of their changing ideas and practices. The original impulse to intervene in cases of family violence was a product of nineteenth-century feminism, and her work makes clear that victims of abuse, mostly women and children, are seen most clearly by social agencies when feminist movements are vigorous.

Gordon's use of casework records to extract historical meaning is dazzling, but her book is not without its structural problems. The best-documented and most thoroughly grounded of Gordon's periods is the Progressive era, and discussions of later decades seem truncated by comparison. Since the late nineteenth century was so crucial in shaping feminist definitions of abuse, I would have preferred more than the sketchy treatment it receives. The bewildering array of topics, periods, and ideas in each chapter makes for difficult reading, and the reader may find it tough going to stay with some of the rather complex arguments that Gordon unfolds. But these are faults characteristic of ambitious books that break new ground. Her study is a pioneering work in the history of social work, family violence, feminism, and the origins of the welfare state.

Gordon takes her title from her final assessment of the victims (and perpetrators) of family violence. Admitting her own and perhaps anticipating her readers' instinctive disapproval of the participants in many family violence cases, she shows us that abusive parenting is a possibility in the lives of everyone. These "heroes of their own lives" were heroic not only because they managed to survive but also because they resisted family violence and in so doing helped to undermine the privacy of the father. Women, both daughters and mothers, who tried to separate from abusive husbands, who reported incest to the authorities to save younger siblings from the same fate, who insisted that wife beating be counted as family violence demonstrate that women can be assertive in the most harrowing of situations, if not, unfortunately, always

victorious in their struggles to protect themselves, their children, or their sisters and brothers.

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SUSAN HOUSEHOLDER VAN HORN. *Women, Work, and Fertility, 1900–1986*. (The American Social Experience Series, number 9.) New York: New York University Press. 1988. Pp. xiv, 232. \$35.00.

This book is an interpretive synthesis of secondary works on two key trends in twentieth-century U.S. history: the decline of fertility and the rise of married women's labor force participation. Susan Householder Van Horn delineates three eras when married women attempted in different ways to balance the changing demands of wage work and motherhood. From 1900 to 1940, fertility declined while labor force participation increased only minimally. During that era, Van Horn argues, "women got less satisfaction from homemaking" (p. 56) as productive functions in the home declined and the status of homemaking diminished. From 1940 to 1960, both fertility and labor force participation rose. Upper-middle-class women spearheaded the baby boom, which Van Horn explains as an innovative attempt "to integrate nostalgia for the traditional family with the benefits of modern life" (p. 99). During the same period, lower-middle-class and working-class women pioneered in expanding married women's entry into the work force, but only during the years before and after childrearing. Women, Van Horn claims, had "a more positive sense of gender" (p. 149) between 1940 and 1960 because they had productive roles in both the home and the workplace. From 1960 to 1986, fertility rates plummeted, and labor force participation soared. Marriage rates dropped, age at marriage rose, and women chose to have fewer, if any, children. For the first time since World War II, large numbers of married middle-class women with preschool children entered the wage labor force. Full-time homemaking no longer attracted women who had rising expectations of economic prosperity and gender equality.

In Van Horn's narrative, the engines propelling change are "modernization" and women's "search for productivity"; the brakes retarding change are "ideological constraints" (p. 75, 207, 124). Within that broader schema, Van Horn focuses on generational differences to explain why different birth cohorts responded to modernization in different ways. Specifically, the size of a birth cohort, its perceived level of prosperity, labor market demand, and the political and cultural climate in the nation at large might stimulate or depress "the taste for fertility" (p. 113) and the decision to enter the work force. When at her best, Van Horn also describes how class, race, religion, and marital status divided each generation.

Van Horn hits her stride in her chapters on the baby boom. She pulls together in straightforward language

the works of demographers, economists, and sociologists. Even here, though, Van Horn's contribution is limited. She neglects much of the recent work in twentieth-century U.S. women's history. Equally troubling, she makes frequent pronouncements about American women's "confusion," "discontent," "satisfaction," and "nostalgia" without supplying an inkling of evidence in support. In general, she omits the anecdotes and quotations that might provide evidence of women's consciousness and bring her material to life.

Still, Van Horn's speculations are often provocative, most notably her positive reassessment of women's experiences in the 1950s. And her outline of the broad sweep of change will be useful for general readers.

JOANNE MEYEROWITZ
University of Cincinnati

JOANNE J. MEYEROWITZ. *Women Adrift: Independent Wage Earners in Chicago, 1880-1930*. (Women in Culture and Society.) Chicago: University of Chicago Press. 1988. Pp. xxiii, 224. \$29.95.

Between 1880 and 1930, the modern woman stepped onto the stage of American history. Sexually aware and economically independent, she was long thought to be a creation of the same processes of industrialization and urbanization that shaped the American middle class. In a brilliant new analysis of independent women wage earners in Chicago, Joanne J. Meyerowitz joins historian Kathy Peiss and others in finding the genesis of modern women's behavior among working-class women rather than among their bohemian urban sisters. Additionally, Meyerowitz deftly sketches the subculture of the urban "woman adrift" and suggests that its meaning as a "potential arena of change" (p. xxii) offers more for our understanding of women's history than the powerful interpretive paradigm of the family economy.

Women came to Chicago not only from abroad but also, like Theodore Dreiser's heroine Sister Carrie, from the countryside. And, like her, many encountered poverty, adventure, and sexual experimentation in the modern city. The category "woman adrift," though, is far more diverse than *Sister Carrie* would lead us to believe. It includes young women as well as old, black women as well as white, rural daughters as well as urban widows. Those women lived as boarders and lodgers and worked as seamstresses, clerks, and prostitutes and in a multitude of other occupations. Their reasons for coming to the city were equally varied, but all faced the "hardships of living apart from family" in a world in which "housing and job markets operated on the assumption that women lived with kin" (p. 20).

The exceptions to that assumption were sexual service workers—masseuses, cabaret dancers, prostitutes. The price those women paid for better wages was high—the ever-present risk of violence and venereal disease. Their activities shaped reformer strategies and helped change the popular image of the "woman

adrift" from an innocent in the city to a sexual sophisticate.

The discourse about the "woman adrift" that filled romance novels, reform writings, and vice reports replaced the "Victorian angel" with the "jazz baby," a figure embraced, Meyerowitz claims, by modern mass culture industries as a symbol of the "vitality and appeal of the modern age" (p. 126). The contrast between those images—the one asexual, the other quintessentially sexual—reflected the declining power of reformers to shape cultural images and mirrored larger social changes from the family orientation of rural society to the anonymity of the city.

Well presented, gracefully written, and mercifully free from the jargon that often plagues social history, this book is a welcome addition to literature in women's, urban, and black history. Meyerowitz re-creates the material world inhabited by Chicago's wage-earning women through a meticulous map of rooming districts, which were subdivided by race and ethnicity and marked by a phenomenally high turnover rate. Through the creative use of census data and the synthesis of literary and social science evidence, she has constructed a virtual geography of urban women's work. Ultimately, though, the book's greatest contribution is in the area of sexuality. Meyerowitz provides answers to, rather than speculation about, questions regarding how and when sexual behavior changes, and she has moved woman as active agent of historical change to the forefront of her analysis.

ANN SCHOFIELD
University of Kansas

JACQUELINE VAN VORIS. *Carrie Chapman Catt: A Public Life*. New York: Feminist Press. 1987. Pp. viii, 307. \$24.95.

The subtitle of Jacqueline Van Voris's biography of Carrie Chapman Catt highlights a problem that the author notes; very little documentation exists on Catt's private life. Thus, we do not have a traditional biography that helps us understand a subject's public and private life as documented in primary sources; rather, we have the story of one woman's work for suffrage and peace for over a half-century, as documented through her published writings, her speeches, and the records of the groups she organized.

Carrie Lane was born in 1859. The first twenty-six years of her life are covered here in ten pages, reflecting the scarcity of sources about her early life and marriage to Leo Chapman. That chapter, however, includes the inspiration for her later activism: she did not understand why her mother was not able to vote in 1872.

In the mid-1880s, the Chapmans coedited the *Mason City Republican* (Iowa), and Carrie used her column to write about the importance of organizing to achieve suffrage. She never stopped proclaiming the importance of organizing. As president of the National

American Woman Suffrage Association (NAWSA) and the International Woman Suffrage Alliance (IWSA), as organizer of the League of Women Voters and the Committee on the Cause and Cure of War (CCCW), she was motivated by Herbert Spencer's belief that society could evolve to a higher plane.

Van Voris spends half of the book detailing Catt's efforts on behalf of suffrage both in the United States and abroad. Using various sources to document Catt's trips around the world in 1911–12 and to South America in 1922–23 and her work with the IWSA, the author paints a fascinating picture of women's status internationally in the early twentieth century. This is an important supplement to the usual U.S.-centered approach to suffrage. The descriptions of Catt's peace work in the 1920s and 1930s, although not as tightly organized as other sections, clearly show that men were not the only ones talking about foreign policy. But we do not learn how Catt felt about more controversial issues, especially birth control. Nor is her relationship to Alda Wilson, her companion of nineteen years, mentioned until a comment that Wilson was with Catt when she died on March 9, 1947.

This book is based on extensive research and has an impressive bibliography and comprehensive footnotes. It would have benefited, however, from more attention to details that may be confusing to readers who know little about Catt or her era. For example, individuals too often come into the story without introduction. The women's organizations that cooperated with the CCCW are not identified in the text and can only be ascertained by checking a later footnote. How did Catt know the Roosevelts well enough to stay in the White House in 1935? A later footnote cites a letter from Franklin Roosevelt, noting that their friendship began in New York in 1911. Despite these frustrations, this is a welcome volume, reminding us once again of the battles fought by our predecessors to guarantee our rights today.

BARBARA J. HOWE
West Virginia University

ILENE PHILIPSON. *Ethel Rosenberg: Beyond the Myths*. New York: Franklin Watts. 1988. Pp. 390. \$18.95.

Ilene Philipson has written an innovative biography of Ethel Rosenberg that adds new insight to one of the most controversial episodes in recent American history. Starting from the "myth" of Ethel Rosenberg, her heroic—or ill-advised—effort to maintain steely self-control during her trial, imprisonment, and execution, Philipson reveals a deeply troubled woman whose courtroom persona was consistent with her personal history. Rejected by a cruel and bitter mother, Rosenberg turned to acting in her youth to escape poverty and family conflict and displayed an ability to separate herself from unpleasant events by creating a persona of disdain and indifference. Ethel's refusal to show emotion to her persecutors, Philipson argues, represented

her lifelong strategy for dealing with pain, not just her acceptance of a Stalinist ethic of loyalty to the cause.

In presenting a psychological, rather than a political, explanation of Ethel's actions, Philipson relies heavily on interviews with Ethel's two therapists and her friends and neighbors. The portrait that emerges is that of a person who found the details of her private life excruciatingly difficult. Ethel not only was the doormat of her own family but also found motherhood a source of unrelieved grief and pain. Her inability to deal with her hyperactive first child made her an outcast among her neighbors (many of whom shared her left-wing politics) and sent her to seek psychiatric help at a time when psychiatry was frowned on in the Communist subculture. Except for the love of her husband, Julius, whose passion for her was boundless and reciprocal, Ethel's life was plagued by rejection as well as by poverty and disappointment. Only politics and art (singing and theater) offered escape from her misery.

Although Philipson does not present any new evidence about the case that led to the Rosenbergs' execution, her portrait of the Rosenbergs' life puts the prosecution of these two working-class Communists in chilling perspective. Even in their own narrow Communist world, the Rosenbergs were losers. Julius was so inept as an engineer that he could not find a job; his business ventures all ended in failure; he was constantly enmeshed in money troubles with his partners and relatives. Ethel could not manage her children, found it impossible to clean her house, and had to march off secretly to a psychiatrist for help with her problems. To charge two people so beaten down by the details of daily life with "stealing the secret of the atomic bomb" shows a society so caught up in political hysteria that it discarded the simplest standards of common sense and decency, not to say its norms of jurisprudence. Julius may have been involved in small-time espionage, but presenting him and Ethel as centers of a master spy ring that jeopardized "millions of lives" was the moral equivalent of accusing Bukharin of being an "agent of Hitler" (p. 306). More than anything I have ever read about the Rosenberg case, this book reveals their execution as a shameful episode in American history.

Predictably, Philipson's book has become the focal point of controversy. The Rosenbergs' sons, who initially supported the venture, threatened legal action to keep it from coming into print and prevented Philipson from quoting or paraphrasing Ethel's unpublished letters. At times, the book seems more like an imaginative reconstruction than a work of history because the footnotes are so thin; much of the material comes from the author's interviews with people who refused to reveal their names. But despite problems of evidence, Philipson's portrait of the Rosenbergs seems credible to me. Like E. L. Doctorow in *The Book of Daniel*, Philipson sees the Rosenbergs as small-time Communists, left behind by postwar prosperity, who found a moment of heroism and transcendence as objects of a public ritual

sacrifice by the American government. For Ethel and Julius, the case offered an opportunity to escape the pedestrian frustrations of their lives and become actors on a world stage; they played that role with an unflinching courage that shamed and frustrated their captors and provided a model for other radicals enmeshed in the ordeal of McCarthyism.

MARK NAISON
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LAWRENCE A. CREMIN. *American Education: The Metropolitan Experience, 1876–1980*. New York: Harper and Row. 1988. Pp. xiii, 781. \$35.00.

Lawrence A. Cremin's three-volume history of education in America, of which this is the final installment, has unveiled itself in splendid isolation from most scholarship in the field. For many years Cremin has been content to go his own way. While offering potential rewards in originality, that tactic has been defused of its promise by failures in analytical imagination. The present volume is the best of the three. There is more effort to provide some serious social history and less reliance on the scattershot inclusion of biographies of educational leaders. But again the lengthy book has no real bite.

The central aim of Cremin's history is to assert that education means more than schooling. Cremin defines education as "the deliberate, systematic, and sustained effort to transmit, evoke, or acquire knowledge, values, attitudes, skills, and sensibilities, as well as any learning that results from that effort, direct or indirect, intended or unintended" (p. x). That means that an enormous range of institutions is actually involved in education, from churches to television. Cremin's third volume pursues a great number of those institutions over the last century, and that requires so much space to do even superficially that schooling itself has had to be nearly left out. The book has everything but the main thing.

Cremin thus devotes chapters to such discrete topics as the history of Protestant home missionary and social service agencies, their Catholic and Jewish equivalents, progressivism as an intellectual and educational movement, private and governmental agencies involved in the rescuing of children (from the Parent-Teacher Association to the Civilian Conservation Corps to Head Start), the mass media, vocational training in farm and factory, and universities. An usually attractive chapter, "Places of Culture," deals with bookstores, libraries, and museums. Toward the end of the book, Cremin includes two chapters on New York City (admitting that it has been arbitrarily chosen), including the lives of some random New Yorkers to show that all of them, from saint to criminal, have been influenced by their educations. The chapters on ideas are almost entirely devoted to progressivism, as if little else existed. Diane Ravitch, in *The Troubled Crusade* (1983), sees educational progressivism as moribund by the 1950s, but for

Cremin it is instead everywhere dominant in recent times. A final general chapter reaches such conclusions as "popularization and multitudinousness . . . were no less characteristic of American education during the twentieth century than they had been in the nineteenth" (p. 650); American education has become increasingly didactic, despite the move away from *McGuffey's Reader* (Cremin gives little evidence to back that claim); and recent worry over a decline in standards is exaggerated (he gives more to support that).

Cremin presents these detailed accounts of organizations and ideas as a kind of paean to pluralism. Qualitative criticisms are made from time to time, for instance regarding television, but they are brief and usually banal, and the main thrust of the presentation is the celebration of diversity—mere wonder that so many various things could exist, and do so side-by-side. (Ethnic diversity is treated with respect, aside from the repeated use of the loaded term Americanization without distancing quotation marks, but given relatively little space.) Throughout, Cremin leaps freely from one time period to another, never lingering to summon the flavor of any era.

Cremin's definition of education, although unusually broad for someone from a school of education, is bound to seem somewhat narrow and artificial in the context of concern over cultural transmission. Why limit one's focus to the "deliberate, systematic" effort to transmit values or skills? Missing is the richer idea of acculturation or socialization—the teaching of habits and ideas by parents and peers. Missing, too, is the notion of an anti-intellectual peer culture, at war with the Sunday school from Mark Twain's day to our own. Cremin presents his diverse educational agencies in a vacuum; they never rub up against limits or fight for the possession of the minds of American youth. Cremin's avoidance of all sharp-edged notions of conflict makes his story seem unreal.

Cremin has been deeply influenced by Daniel J. Boorstin, in the subtitles of his volumes and in his emphasis on variety rather than conflict (to his credit, Cremin does not echo Boorstin's narrow nationalism). But Cremin lacks Boorstin's grace. He moves from one topic to another in a manner that seems arbitrary and choppy. Specialists in the history of education will find Cremin's latest work too unprovocative and miscellaneous to excite great interest; those seeking a broad account of American culture in the last century, written from a utilitarian point of view, would do better to read Boorstin.

LAURENCE VEYSEY
Lahaina, Hawaii

MARC SCOTT MILLER. *The Irony of Victory: World War II and Lowell, Massachusetts*. Urbana: University of Illinois Press. 1988. Pp. xi, 233. \$24.95.

This book should be read in two ways. Marc Scott Miller's primary objective, as I understand it, is to add

to the valuable historical studies about the domestic side of the war. Read in another frame, Miller provides a much-needed in-depth examination of the human face of deindustrialization in an old urban center.

The book is based on approximately seventy oral history interviews conducted by the author. That evidence is complemented by archival research in newspapers and government documents. The volume has been organized into eight chapters, most of which direct attention to a specific segment of wartime life (for example, women, children, the mill elite). Sometimes the author succeeds. The chapter entitled "Working-Class Life" reads like a well-crafted ethnography. That the book does not sustain that high standard probably reflects the evidence on which it is constructed.

The author's thesis offers no surprise. Lowell temporarily prospered during the war-induced economic boom, only to quickly decline in peacetime. Miller documents the plight of individuals, at times evocatively, within the context he has drawn. He also recognizes the historical sources of the underlying dilemma.

Readers stand forewarned about historiographic lapses. First is Miller's failure to incorporate into his findings Philip J. Funigiello's important monograph, *The Challenge to Urban Liberalism: Federal-City Relations during World War II* (1978). Second, no one should write about Lowell without consulting Thomas Dublin's prize-winning volume, *Women at Work: The Transformation of Work and Community in Lowell, Massachusetts, 1826-1860* (1979).

MICHAEL H. EBNER
Lake Forest College

MICHAEL H. EBNER. *Creating Chicago's North Shore: A Suburban History*. Chicago: University of Chicago Press. 1988. Pp. xxx, 338. \$29.95.

Michael H. Ebner's purpose is to discover "when, how, and why people came to view the North Shore as a self-conscious network premised upon common assumptions and shared values" (p. xvii). To do so, he investigates the evolution of eight communities from their founding through the early twentieth century. Several of these places developed a distinctive identity: Evanston, for example, as a "sanctified" village because of its temperance laws; Wilmette because of its tradition of civic participation; Winnetka for its commitment to education; Highland Park for its design, which was platted by H. W. S. Cleveland and Walter French; Lake Forest as a haven for the socially prominent. Three other communities included in this study—Kenilworth, Glencoe, and Lake Bluff—similarly aspired to status as prestigious suburbs, while Highwood, Fort Sheridan, and North Chicago never conformed to the moral geography of the North Shore because of nearby military installations. Because Ebner perceives suburbanization as part of the process of urbanization, following the lead of Kenneth T. Jackson and H. J.

Dyos, he devotes considerable attention to the North Shore's relationship to Chicago.

Like Jackson, Ebner analyzes the introduction of new transportation technologies that made suburbanization possible, as well as the matrix of cultural ideals that enshrined the single family home as the centerpiece of the American dream. The unique contribution of this book is its depiction of the tension between attachment to community and the gradual emergence of a North Shore identity. The Onwentsia Club (founded in 1895), for example, reinforced Lake Forest's self-image as a place apart, as did educational, social, and cultural institutions in other communities. Thus, when the North Shore Improvement Association promoted the creation of Sheridan Road and a second railroad line in 1889, the tradition of localism triumphed over intermunicipal cooperation. Residents opposed those projects because of the cost, because they feared the impact of new technologies on their communities, or because they wanted their suburb to control its own destiny. In the early years of the twentieth century, however, several developments portended the emergence of the North Shore as a distinct regional network. Ravinia Park (established in 1904) became an institution that sustained a common identity among the eight North Shore communities. The increasing pollution of Lake Michigan, which threatened the water supply of the North Shore, similarly forced residents to appreciate the advantages of intermunicipal cooperation. The North Shore Sanitary Association, established in 1909, spearheaded the creation of a sanitary district that met the needs of Lake (though not Cook) County, a tacit admission that a North Shore identity had not yet supplanted residents' primary attachment to community.

In demonstrating the evolution of this suburban network, Ebner enriches his analysis of communities and institutions by adding biographical profiles of the individuals whose actions shaped the North Shore. Among these are people as diverse as architect Daniel H. Burnham, educator and temperance activist Frances E. Willard, Henry Demarest and Jessie Bross Lloyd, the Farwells of Lake Forest, Catholic priest William Nestraeter (who mediated between his German-American congregation and its temperance-minded neighbors), as well as the scions of numerous Chicago fortunes who individually or collectively migrated from that city to its northern suburbs. Drawing on generations of local historical works, a number of recent scholarly monographs devoted to Chicago, a thorough grounding in secondary studies of urban and social history, and painstaking research, Ebner has produced an engagingly written, lavishly illustrated book. Critics might find fault with the author's reliance on *Who's Who* and the *Social Register* as indexes of status or question the degree to which a regional point of view coexisted with attachment to community, but this is an important work, one that adds significantly to our

understanding of the dynamic of urban and suburban growth.

DAVID SCHUYLER
Franklin and Marshall College

DOUGLAS L. SMITH. *The New Deal in the Urban South*. Baton Rouge: Louisiana State University Press. 1988. Pp. 287. \$29.95.

According to Douglas L. Smith, the impact of the Great Depression and the New Deal caused southern cities to become somewhat more like their northern counterparts and like the modern metropolitan areas they would later become, yet that impact did not depose the reigning conservative ethos, regional values, and commercial-business elite that maintained traditional racial and socioeconomic patterns in the urban South of the 1930s. As such, Smith's book buttresses the scholarship of Blaine Brownell and David Goldfield, emphasizing the uniqueness of the South's cities in the twentieth century. And it augments the current prevailing interpretation that rejects both the view of the New Deal as a watershed that fundamentally changed the American system and the New Left argument that the so-called "Roosevelt Revolution" neither altered nor improved American life significantly.

While never minimizing the shortcomings of the New Deal, Smith credits it with promoting municipal service activities and with advancing the principle of public welfare and an urban consciousness in the South. In his discussion of Atlanta, Birmingham, Memphis, and New Orleans, the four largest southeastern cities of the time, Smith demonstrates how the various federal assistance programs of the 1930s proved both the worth of welfare projects and the necessity for urban planning as well as for permanent local welfare institutions. Similarly, despite mighty forces arrayed against it, the New Deal housing effort provided jobs and shelter for many thousands in need, while it also stimulated local agencies to confront the problems of urban blight and poverty as public responsibilities. The growth of organized labor and of an increasingly militant civil rights movement, moreover, prodded the four cities to accept some changes in the traditional southern way of life. Gradually, such values as social order and fiscal parsimony grudgingly accommodated new concepts of community well-being.

This book is hardly exciting or easy reading. It is bereft of a single vivid quotation or colorful character, and the abundance of data overwhelms the minimal effort at analysis and interpretation. Too often, Smith fails either to grapple with the meaning of the evidence he presents or to articulate the judgments he has reached. Yet, Smith's extensive and careful research in the records of New Deal agencies, the manuscripts of southern municipal leaders, and the relevant daily, labor, and black newspapers gives us a gold mine of vital information on the workings of the New Deal at the local level. His industriousness and careful objec-

tivity make his book a welcome addition to the growing body of literature on southern cities. Perhaps it will stimulate other monographs that can match the excellent works on the New Deal and the agrarian South.

HARVARD SITKOFF
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FRED I. GREENSTEIN, EDITOR. *Leadership in the Modern Presidency*. Cambridge: Harvard University Press. 1988. Pp. 430. \$29.95.

In this uneven collection, nine authors discuss the presidents who have served since 1933. Five historians who have written notable books on U.S. presidents here reiterate or reconsider their earlier views. Although William E. Leuchtenburg remains impressed with Franklin D. Roosevelt's legislative leadership and shrewd use of the media, he concedes that Roosevelt may have contributed to what Arthur M. Schlesinger, Jr., calls the "imperial presidency." Alonzo L. Hamby writes that Harry S. Truman transcended provincialism and an erratic personality to lead a largely successful administration, marked especially by the "dazzling achievement" (p. 70) of bipartisan foreign policy. Still convinced that Dwight D. Eisenhower is insufficiently appreciated, Fred I. Greenstein presents Eisenhower's sensible reflections on the nature of leadership and demolishes the notion that he brought a "military" style to the White House. Chronicling the John F. Kennedy administration's underrated legislative record, ambivalent foreign policy, and strained alliance with the civil rights movement, Carl M. Brauer stresses that Kennedy himself was "inspirational in a way that few Presidents have been" (p. 109). After briefly showing that the Great Society offered "something for everybody" (p. 135), Larry Berman concentrates on Lyndon B. Johnson's conduct of the Vietnam War, concluding that Johnson's suspicion, hypersensitivity, and penchant for secrecy kept him from understanding the conflict he escalated.

The two most interesting essays present synopses of larger works in progress. Joan Hoff-Wilson notes Richard M. Nixon's fondness for acting boldly in real or imagined crises, adding that such behavior transformed Watergate from a "second-rate, second-story job into a first-rate constitutional crisis" (p. 168). Yet she deliberately slights both Nixon's personality and the Watergate scandal to concentrate on his efforts to reorganize and centralize government administration, social services, and foreign policy. Though properly skeptical of his foreign policy, she gives Nixon too much credit for extensions of the welfare state that occurred during his term. Erwin C. Hargrove rightly places Jimmy Carter in the southern Progressive, as opposed to Populist, tradition, sees his obsession with "comprehensive" solutions as a major strategic handicap, and acknowledges important similarities between the Carter and Ronald Reagan administrations. This attempt to move beyond prevailing clichés is admirable

and generally successful. But Hargrove misinterprets the impact of Carter's "born again" religion, offers unduly generous assessments of his energy policy and the Camp David Accords, and takes at face value Carter's claim to stand above politics.

The weakest essays deal with Gerald R. Ford and Reagan. After recounting the major decisions and internal squabbles of Ford's "healing" administration, Roger Porter concludes that the president used different leadership styles on different occasions. Porter makes no further effort to appraise Ford's record, perhaps because he relies too much on self-serving memoirs and oral histories; apparently he examined none of the contemporary records now available at the Ford presidential library. Quoting extensively from interviews with White House speech writers, William K. Muir, Jr., analyzes Reagan's rhetoric instead of his policies. This approach might be productive if the analysis were less thin. According to Muir, Reagan used religious themes, revived the "traditional notion of evil" (p. 273), which he temporarily applied to the Soviet Union, and stressed national "teamwork." From the reverent explication here, we might mistake Reagan for an original theorist on a par with Herbert Hoover if not John C. Calhoun. Unfortunately, Muir does not seem to know, for example, that emphasis on national unity has been a rhetorical convention since the Progressive era and that Eisenhower and Nixon promoted "teamwork," too.

In introductory and concluding essays, Greenstein tries hard to draw the articles together. Yet the book as a whole seems internally contradictory because, with the exception of Berman, the authors praise their respective presidents, presidents who would not necessarily have praised one another. For example, if readers hail Roosevelt, Truman, and Johnson for creating a welfare state in chapters 1, 2, and 5, how can they share the authors' enthusiasm for Eisenhower, Ford, and Reagan, who try to shrink the welfare state in chapters 3, 7, and 9? Moreover, although iconoclastic books on the presidency are scarce, Greenstein's selection of contributors so fond of their respective subjects makes this one unusually bland. We might have learned more if Greenstein had asked Leuchtenburg to write on Reagan (whom he has elsewhere compared to Roosevelt), asked Hamby to write on Ford (who compared himself to Truman), and assigned Roosevelt to Hoover's biographer George Nash. As it now stands, this volume may appeal most to optimists who believe that there has never been a really bad president.

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NIELS AAGE THORSEN. *The Political Thought of Woodrow Wilson, 1875-1910*. (Supplementary Volumes to the Papers of Woodrow Wilson.) Princeton: Princeton University Press. 1988. Pp. xiv, 272. \$34.50.

Politics, religion, and pathology have dominated modern scholarship on Woodrow Wilson. In his many works, especially in *Woodrow Wilson and the Progressive Era, 1910-1917* (1956), Arthur Link has concentrated on Wilson as governor of New Jersey, party leader, president, and diplomat. John Morton Blum, in *Woodrow Wilson and the Politics of Morality* (1956), stressed the religious framework within which Wilson's politics operated. Alexander George and Juliette George, in *Woodrow Wilson and Colonel House* (1956), focused on the pathology of Wilson's family situation and the psychological consequences of his relationship to his father. Some of the best recent work clearly derives from these books: John M. Mulder's *Woodrow Wilson: The Years of Preparation* (1978) is an excellent example of works in the tradition of Blum, incorporating recently published primary sources to come to conclusions friendlier to Wilson than Blum's were. Edwin A. Weinstein, in *Woodrow Wilson: A Medical and Psychological Biography* (1981), has displaced psychobiography and substituted instead an emphasis on strokes and Wilson as physically rather than psychologically ill at crucial times.

What none of these books has done is examine Wilson, the public administrator, as intellectual. Niels Aage Thorsen has filled this gap in a thorough and competent way. Writing in the tradition of Link, he has all but taken for granted that Wilson was chiefly a political animal. He was neither neurotic nor ill in any crippling way, and religion, although important in his life, was not the strait jacket that it seems to be in other volumes. At times, Thorsen conducts a running debate with Mulder, on page after page showing how the same letters and speeches that Mulder used to demonstrate the primacy of religion really deserve a more political reading. Both sides of this debate have merit, and both books are well done. I am firmly committed to the assumption that religion and morality were primary, but those who are more secular and political in their views of Wilson will be happy with Thorsen's book.

Of necessity, Wilson scholars tend to repeat each other. Many pages of this volume could have been shorter, and a significant number of block quotations needed condensing or paraphrasing. This said, the book makes an imaginative case for Wilson as a pioneer of public administration. It reviews the criticisms that Wilson's work has received and demonstrates that a close study of Wilson's intentions, and the context of his terminology, reveals a mind more original than many of us have thought. Thorsen makes an especially appealing case for Wilson's *Congressional Government* (1890) and has some fresh information about Wilson's European sources. Few political scientists have received such detailed study.

The major problem with this volume is that, careful as the author is in his phraseology, he still falls into the elementary fallacy of assuming that religiosity in Wilson meant in practice an interest in theology. "While it is no doubt true that Wilson's views were deeply informed by Calvinist moral psychology, his writings

and the record of his readings contain no evidence of a sustained interest in Calvinist theology. Wilson's intellectual concerns were simply oriented toward political rather than religious purposes" (p. 167). I know of no historian who has written as if theology were the great passion of Wilson's life. The point is that his Presbyterianism gave him a limited understanding of human behavior, the political process, and diplomatic controversies. Wilson repeatedly overcame his rigidities to become a revered teacher, admired governor, and effective president, but in times of stress and illness he seemed to collapse into postures unworthy of greatness. The consequences proved damaging to Princeton, to progressive reform, and to the negotiations at Versailles. Theology was not the problem.

Thorsen also overdoes Wilson's stature as an academic. He was competent rather than seminal. "Wilson's contribution is of an order that justifies a comparison with the political science that lay behind the creation of the Constitution" (p. 228), Thorsen writes, going on to compare Wilson's views to Madison's and finding them comparable in importance as well. I will stick with Madison, but, for those who think otherwise, this book has all the evidence effectively marshaled.

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DUANE TANANBAUM. *The Bricker Amendment Controversy: A Test of Eisenhower's Political Leadership*. Ithaca: Cornell University Press. 1988. Pp. xiii, 263. \$34.50.

During the cold war era of McCarthyism, isolationists, conservatives, and a great number of assorted states-righters, anti-New Dealers, and xenophobes became convinced that the United Nations (UN) and liberal Democratic presidents were excessively invading the rights of states and individuals. Their fears originated in the case of *Missouri v. Holland* (1920), in which the Supreme Court held that a treaty regulating the hunting of migratory birds was the supreme law of the land. Ultimately, these dissidents believed, this decision would leave virtually meaningless the rights of states protected by the Tenth Amendment. Their apprehension increased after World War II when the United States joined the UN and that body adopted the Genocide Convention. The UN also began deliberations on the Declaration of Human Rights (ratified by the United States in 1988), which included social and economic concerns and which many thought might undermine American sovereignty. These groups were also quite agitated over the use of executive agreements in the destroyer deal and at the Teheran, Yalta, and Potsdam conferences, because those agreements bypassed the treaty process. Senator John Bricker and the American Bar Association decided that a constitutional amendment was necessary to prevent further breakdown of the American political system.

As reported from committee, Bricker's amendment contained three parts. The first provision prohibited treaties that conflicted with the Constitution. The second article allowed a treaty to become effective as internal law in the United States only through legislation that would be valid in the absence of a treaty. And the third article gave Congress power to regulate all executive and other agreements with any foreign power or international organization. All such agreements were subject to the limitations imposed on treaties by the third article. Dwight Eisenhower and his advisers had no problem with the first provision but opposed the second; the clause would have affected treaties permitting Americans to own property in other countries. Moreover, they were adamant against the third stipulation, which they believed would place the president in a strait-jacket in conducting foreign policy.

The administration worked with Senator Walter George to develop an alternative. They found, however, that even George insisted on regulating executive agreements. His substitute amendment contained Bricker's first clause, which Eisenhower would support, but included another that said "an international agreement other than a treaty shall become effective as internal law in the United States only by an act of Congress." This was the proposal on which the Senate finally voted, and Eisenhower had to throw the full weight of the White House against it to get it defeated. The roll call vote was sixty to thirty, exactly the necessary two-thirds. At that point Harley Kilgore staggered onto the floor, brought either from a tavern or from trying to sleep off the effects of a binge, and he or one of his aides called out nay and killed the amendment.

Duane Tananbaum carefully traces the emergence of the movement to support Bricker's amendment, as well as Eisenhower's preliminary, vague opposition to it, which hardened once he discovered the amendment might pass. The book is thoroughly researched, carefully crafted, and well written. The author demonstrates that the broader issues raised by this amendment, namely the relationship between the United States and the UN, between the states and the national government, and between president and Congress, are still relevant today. Tananbaum points out that Eisenhower's realization of the danger of the situation almost came too late. His evidence does not reveal a "hidden-hand" presidency so much as a president who knew how to choose a good staff and delegate responsibility. It would be of interest and significance to know why, when the vote was expected to be extremely close, Eisenhower's aides told "John Sparkman and others" that they could vote for the amendment to satisfy their constituents (pp. 188-89). These quarrels aside, this is a valuable contribution to our knowledge of the Eisenhower era.

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GARY L. MCDOWELL. *Curbing the Courts: The Constitution and the Limits of Judicial Power*. Baton Rouge: Louisiana State University Press. 1988. Pp. xiv, 214. \$27.50.

Gary L. McDowell, an articulate advocate of the original intention theory of judicial interpretation, adds a provocative new dimension to the current debate over "judicial activism." His study, however, is not a thorough historical examination of the evolution of federal judicial power. Instead, it is an unabashed invocation of history for the purposes of curtailing what McDowell believes are the excesses of today's federal courts.

Recent congressional attempts at court curbing have failed, McDowell concludes, because they either have sought to limit specific judicial powers or have relied on the unwieldy amending process to correct controversial Supreme Court decisions. A far better approach would be to have Congress, which McDowell charges has shirked its duties, strike at what he views as the heart of the so-called judicial activism problem: the procedures governing pleading before federal courts. McDowell urges Congress to undo much of the New Deal legislation (particularly the Declaratory Judgments Act of 1934, the adoption of the Rules of Civil Procedure of 1938, and the Administrative Office Act of 1939) that made federal litigation far easier and federal courts far more open to insular minorities than before. Collectively, McDowell argues, those measures were "an effort to defuse political antagonisms toward federal courts while leaving intact and untouched the substantive heart of the judicial decision-making process" (p. 7). Ever since, he insists, federal judges have played fast and loose with the Constitution.

McDowell devotes significant attention to six specific procedural matters: standing, class actions, intervention or joinder, declaratory relief, consent decrees, and equitable relief. He argues that the federal courts, because Congress has permitted them broad discretion to formulate the standards in each of those areas, have encroached on policy matters better left to the legislative branch. To buttress his case, he flays away at justices and judges such as Earl Warren, Frank Johnson, Arthur Garrity, and Frank Battisti. He charges that each of them substituted their own moral "concern for social justice" for the traditional judicial "concern for constitutionality" (p. 151). A federal judicial establishment held in the thrall of liberalism and welfare statism has made "the constitution . . . what the judges say it is" (p. 7).

The study, although engagingly written, rests on three debatable historical assumptions. First, McDowell contends, "no period in our history has equaled either in duration or intensity the current period of antijudicial sentiment" (p. xiii). McDowell's proposed reforms, of course, have little urgency if there is no impending crisis. Second, the framers of the Constitution and of the Fourteenth Amendment believed that the lower federal courts were political conveniences that Congress could dispose of as it wished. And, third, proce-

dural developments in the federal courts of post-New Deal America have had invidious results.

In each instance, McDowell's effort to cloak his reform agenda in the trappings of historical legitimacy appears overwrought. The Jeffersonian crisis over the courts in the early nineteenth century, which resulted in the impeachments of Judge John Pickering and Associate Justice Samuel Chase and in which Chief Justice John Marshall himself was attacked, posed a far greater threat to effective federal judicial power than today's largely genteel dialogue among academics. Moreover, the generation that wrote the Constitution was closer in 1789 (the year in which Congress passed the nation's most important judiciary act) to understanding the necessity of having lower federal courts than we are today. The same can also be said of the congressmen who passed the far-reaching judiciary acts of 1869 and 1875, since those legislators were obviously aware that only through a more powerful lower federal judiciary would it be possible to implement the Fourteenth Amendment.

McDowell also exaggerates the effects of the procedural devices with which he takes issue. With a majority of current lower court judges appointed by a Republican administration committed to judicial restraint and a jurisprudence of original intention, the crisis (if there ever was one) must surely have passed. In this regard, McDowell might also have appropriately taken account of the increasing appeals by social and political minorities to the highest courts of the states, whose judges are now elevating the ceiling of civil rights and civil liberties above the floor laid by post-New Deal federal judges. The new Republican majority in the federal courts appears to be doing just what McDowell urges, and the result is that one of the most important historical avenues by which minorities have claimed their civil rights and liberties is closing.

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CHARLES H. SHELDON. *A Century of Judging: A Political History of the Washington Supreme Court*. Seattle: University of Washington Press. 1988. Pp. xii, 379. \$35.00.

State courts are now in the limelight. When the U.S. Supreme Court under Chief Justice Warren Burger retreated from the liberal activism of the days of Chief Justice Earl Warren, its rationale (when the claimed civil rights were not denied altogether) was that state courts were the proper bodies to decide such questions. Justice William Brennan, speaking for the defeated liberals, summoned state courts to rise to the occasion, while Chief Justice Burger prayed aloud that they not mimic the Warren Court. Because state constitutions contain many of the same promising generalities as the federal constitution, the willingness of state judges to take the lead is of great moment. State constitutional law has emerged from long eclipse and is once more appearing in law school curricula. State supreme

courts, the final authoritative expositors of state constitutions, are now being scrutinized as never before.

Charles H. Sheldon has produced what he calls a "political history" of the Washington State Supreme Court to mark the occasion of its first centennial. As disarmingly described in the first paragraph, the work "is historical simply because it records events over time and identifies the persons responsible for those events"; it is political because it provides "the meanings to be attached to the sequence of events and the significance given the motives of the persons involved" (p. 3). In fact, the book is not so much political history as political science. Almost two hundred of the three hundred forty pages of text are devoted to judicial recruitment. The selection of every judge—a total of seventy-seven in all—is told in great (and occasionally confusing) detail. The trend of development is, however, clear and unsurprising: from nomination and election as political partisans to bureaucratized selection heavily influenced by the organized bar and belatedly ratified at the polls. Profiles of the judges reveal a comparable shift from pioneering settlers to Washingtonians who attended the state law school. Following a helpful description of the court's business, five "representative courts," based on the previously described patterns of recruitment, are studied. True to the political science approach, copious tables displaying empirical data are included. A wealth of valuable information is presented, although a few questionable assumptions are hazarded in an attempt to quantify the unquantifiable, such as that longer judicial opinions are likely to be "more thorough" (p. 287).

For a history of a "century of judging," remarkably few cases are discussed; of the unnumbered thousands of opinions filed, fewer than fifty Washington State Supreme Court decisions are cited. Leading cases, including political causes célèbres, are barely mentioned: for instance, a "progressive decision" from 1902 concerning employer liability for industrial injuries (p. 55); a challenge to the business and occupation tax imposed as part of the state's "little New Deal" (pp. 96–97); the bitterly contested issue of public ownership of utilities (pp. 117–23); and the "celebrated 'Seat of Government' case" concerning the location of state offices (pp. 300–01).

Relations with the federal courts are, unfortunately, scantied. There is no organized account of appeals to federal judges, who are dismissed (without empirical data) as "remote and sometimes arrogant" (p. 338). On the crucial issue for the court's second century—the exposition of the state constitution, particularly with reference to civil rights—the discussion is exiguous. What was perhaps the most famous case from Washington ever appealed to the U.S. Supreme Court is briefly and misleadingly described: "Judge Millard's majority opinion in *West Coast Hotel v. Parrish* (300 U.S. 379, 1937) is perhaps a special but important example. Until this 'switch in time that saved nine,' the U.S. Supreme Court's reading of the due process clause of the Fourteenth Amendment to the U.S.

Constitution largely tied the hands of state courts" (pp. 339–40). In fact, of course, while Judge William J. Millard of the state supreme court had courageously staked out state interests exceeding then-perceived federal limits (185 Wash. 581, 1936), it was U.S. Chief Justice Charles Evans Hughes who penned the majority opinion in the U.S. Reports and Justice Owen Roberts whose switch at the time of the court-packing controversy "saved nine."

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BRUCE ALLEN MURPHY. *Fortas: The Rise and Ruin of a Supreme Court Justice*. New York: William Morrow. 1988. Pp. 717. \$25.00.

Few years in the history of the United States have a greater claim to the designation "watershed" than does 1968. During this twelve-month period, the tide turned decisively against the U.S. effort in Vietnam, Lyndon Johnson was driven from the presidential race, assassins murdered Martin Luther King, Jr., and Robert Kennedy, the Democratic party imploded in Chicago, and Richard Nixon captured the White House. It was also the year in which Yale University first admitted women undergraduates, Jackie Kennedy became Jackie Onassis, student rebels occupied Columbia, the North Koreans seized the U.S.S. *Pueblo*, and the Beatles produced *Magical Mystery Tour*. It was, depending on one's point of view, the year the music stopped, the final act of Camelot, the death of liberalism, the twilight of empire, the triumph of conservatism. It was also, Bruce Allen Murphy reminds us in this absorbing new book, the year the U.S. Senate refused to confirm Abe Fortas as chief justice of the United States, the beginning of the end of Fortas's judicial career, and a year that may have fixed the destiny of the Supreme Court for the remainder of the century.

This book is both a minibiography of one of the most powerful American lawyers of the post-World War II period and a study of the personal, institutional, and ideological forces that led to his political destruction between 1965 and 1969. Even without benefit of his subject's personal papers, Murphy succeeds brilliantly in the second part of this project. His blow-by-blow account of the titanic confirmation battle and of the financial scandals that led to Justice Fortas's resignation a year later, based on numerous interviews and extensive research in many archives, is the fullest account we have of these matters. It is likely to become a classic in the literature of politics and the judiciary.

The first quarter of the book, tracing Fortas's life from an impoverished childhood in Memphis to the pinnacle of Washington's legal establishment, is less convincing because of the dearth of personal papers and the author's annoying habit of attributing virtually every decision made by his protagonist to a lust for

power, wealth, and success. Who can doubt that Fortas was ambitious, ruthless, and driven by a healthy self-interest? But it is a bit much to argue that, in his whole New Deal career, he was never "by nature philosophically committed to any viewpoint other than getting ahead" (p. 25). This is far too narrow a view of Fortas's behavior in the Agricultural Adjustment Administration, in the Public Utilities Division of the Securities and Exchange Commission, and in Harold Ickes's Department of Interior, where, among other things, he blocked efforts to further enrich the ALCOA monopoly and to turn the Elk Hill oil field over to Standard Oil. Murphy manifests grudging admiration for the *pro bono* work of the law firm of Arnold, Fortas, and Porter, which continued even during the height of the McCarthy era, when the partners defended many victims of the red scare. Owen Lattimore and Dorothy Bailey get short notices, while the partners' lavish life styles and their many corporate clients are given a careful inventory. The possibility that self-interest and the public interest often merged in Fortas's career seems to have eluded Murphy at times, because he wishes to stress the dramatic tale of a man undone by his own avarice and egoism.

Fortas indeed fashioned the weapons that were turned against him by his enemies in the Senate and in the Nixon administration. The full extent of his political activities in Lyndon Johnson's administration (including speech writing and drafting legislation) never became public, but what did emerge before the Senate Judiciary Committee raised alarm about the separation of powers. Instead of accepting a \$15,000 fee (contributed by five corporate executives with close ties to his old law firm) to teach a seminar at American University, he might have enriched the students' minds for a smaller sum. And what stunned even Fortas's supporters about his notorious consulting contract (later cancelled) with the Louis Wolfson Foundation was not the size of the retainer (\$20,000 per year) but the provision that the payments were to continue for the duration of the justice's life as well as his wife's.

Fortas might have survived those embarrassing revelations, Murphy makes clear, had he not been trapped in even more powerful historical tides: the erosion of Johnson's political capital in the wake of Vietnam and the domestic turmoil of the late 1960s; the president's foolish reliance on old Senate whales, especially Richard Russell and Everett Dirksen, whose influence had become largely symbolic and who betrayed him when the battle heated up; the ability of Senate demagogues, principally Strom Thurmond, to exploit shamelessly the mounting resentment against the decisions of the Warren Court on race, criminal justice, and freedom of expression; and the enmity of one of his own brethren on the bench, Justice Hugo Black. Many of Fortas's wounds were self-inflicted, but they probably were less damaging to his nomination than the bigotry and fear unleashed by the likes of Thurmond, James Eastland, and Robert Griffin. In his survey of Fortas's most notable opinions between 1965 and 1969, Murphy also

confirms what a strong defender of the Bill of Rights this man became and what a jurisprudential disaster his own personal calamity was for a court soon populated by Warren Burger and William Rehnquist.

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RICHARD C. CORTNER. *A Mob Intent on Death: The NAACP and the Arkansas Riot Cases*. Middletown, Conn.: Wesleyan University Press. 1988. Pp. xii, 241. \$25.95.

In October 1919 a bloody "race war" unexpectedly erupted in eastern Arkansas, resulting in the deaths of five whites and over two hundred blacks before federal troops restored order. Terrified local whites insisted that blacks had been plotting a violent uprising against plantation owners, which had been prematurely discovered. In the riot's aftermath, twelve black farm hands were hastily convicted and sentenced to death, while sixty-seven others received prison sentences. No local whites were ever charged. The National Association for the Advancement of Colored People (NAACP) intervened on behalf of the prisoners and, after a difficult five-year struggle, finally won their freedom.

In this fascinating narrative account, Richard C. Cortner reconstructs the riot and the hostile reactions of most Arkansas whites. He reveals that sharecroppers from local plantations, the victims of debt peonage and manipulation by landlords, had formed a union to protect their interests. When a sheriff attempted to break up one of their meetings, he was shot, touching off panic among local whites, who armed themselves and massacred numerous blacks. A courageous Walter White, then a youthful NAACP administrator, boldly traveled to Arkansas and "passed" for white in order to investigate the incident. His findings helped convince the NAACP to enter the case.

Cortner provides a detailed description of the complex legal battle that followed. As its principal liaison within the state, the NAACP relied on Scipio Africanus Jones, a remarkable black attorney who had been born a slave. Moorfield Story, a distinguished Boston attorney, directed the eventual appeal to the U.S. Supreme Court. This unlikely combination of a former slave and a Boston patrician won an unexpected constitutional victory from the Supreme Court in *Moore v. Dempsey* (1923), which established the principle that a mob-dominated trial in a state court was invalid. This ruling constituted an opening wedge for supervision by federal courts of state criminal trials and thus constituted "a milestone in the modern interpretation of the Due Process Clause of the Fourteenth Amendment" (p. 185). Yet Cortner prudently notes that it was not until the 1960s that the federal courts fully applied the procedural safeguards of the Bill of Rights to defendants in state (as opposed to federal) criminal trials.

Cortner has written a solid and quite readable case study. It provides extensive information about the

development of legal strategy and behind-the-scenes maneuvering, pointing out just how fortunate the NAACP was to win its appeal. There are a few minor weaknesses, however. Cortner uses too many long quotations, and occasionally there is too much repetitious material concerning the riot and the major legal arguments. He also fails to investigate very deeply the actual labor conditions on local plantations that created black grievances. Nonetheless, the author has successfully documented both the social and the legal background of this important but sometimes overlooked case. Hence, the book will be of value not only to legal historians but also to scholars of southern and black history.

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JOHN S. SCHUCHMAN. *Hollywood Speaks: Deafness and the Film Entertainment Industry*. Urbana: University of Illinois Press. 1988. Pp. x, 167. \$24.95.

The purpose of this study is to "describe how Hollywood has depicted deaf characters over a period of eighty years and to compare this image with the deaf community as I understand it and have experienced it" (p. ix).

John S. Schuchman concludes that the film and entertainment industries have dealt with deafness in a manner similar to their stereotyped treatment of ethnic and racial minorities, that is, those industries have contributed to the misunderstanding of deafness and to the attitudes that have permitted discrimination against deaf persons.

Schuchman also concludes that deafness carries an additional stigma. Many people view deafness as pathological. Motion pictures and television shows reinforce the often mistaken belief that nonspeaking deaf people can talk if they only try and that deafness can be cured by a traumatic event. Hollywood has a tendency to focus on only one deaf character in order to show how that person interacts with the hearing world. That tendency overlooks the valuable support networks of the deaf community.

Silent film, however, with its captions and exaggerated pantomime, did not discriminate against deaf audiences. Deaf persons participated in silent film audiences on a comparatively equal basis with their hearing peers, as drama, comedy, and the news unfolded in front of them. No assistance was necessary. Since the advent of talking films in 1927, most films have not provided captions or signing interpreters for the deaf. Closed-captioned television has been available only for the past decade.

In the past Hollywood produced some excellent films inspired by deafness. Among them were *Johnny Belinda* (1948), *The Miracle Worker* (1962), *The Heart Is a Lonely Hunter* (1968), and *Children of a Lesser God* (1986). *Children of a Lesser God* was the first major movie to

employ only hearing-impaired artists to play deaf characters. It marked somewhat of a breakthrough for the deaf community. Paramount Pictures distributed more captioned prints of the film than any previous release from any studio.

One of the most interesting and useful parts of this book is the author's formal filmography on deafness. It is the most complete to date, and it provides a useful tool for scholars who wish to pursue further research on the subject. The filmography is divided into two categories: motion pictures produced for theatrical viewing and television programs. In addition, a final section lists sign-language motion pictures produced for the deaf community that were not made in Hollywood. All films are listed chronologically, which is a great convenience for the reader.

A few minor problems exist. A curious omission is the author's failure to discuss the John Tracy Clinic, founded in 1942 by Louise Treadwell Tracy, a member of the Hollywood community. That educational center for preschool deaf children and their parents has been an important part of the deaf community's response to beginning communication skills. Also, although the book has chapter endnotes, there is no bibliography. Finally, the page numbering is incorrect and confusing in the early part of the book.

But those are minor flaws. This study is logically organized, nicely balanced, and well written. In short, it is the best treatment of the subject and very likely will remain so for years to come. It also proves that film history is changing as we seek to discover the meaning of movies and their important place in our culture.

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WALDO HEINRICHS. *Threshold of War: Franklin D. Roosevelt and American Entry into World War II*. New York: Oxford University Press. 1988. Pp. x, 279. \$21.95.

Nearly a half-century after Franklin Roosevelt presided over U.S. entry into World War II, historians continue to examine how war came and what role Roosevelt played in its coming. Waldo Heinrichs tackles that job not just by placing Roosevelt's decisions of March–December 1941 in their global context but by integrating them into what Heinrichs calls "the military side—intelligence and operational capability as well as strategy" (p. viii). In considerable detail, Heinrichs describes war news from all over the world, conflicting and confusing intelligence reports, strategic views put forward by the armed forces, and constraints of material and logistics operating on the president. The result is a study that puts the reader behind Roosevelt, looking over his shoulder at the information the president saw.

Month by month Heinrichs takes the reader through the chain of decisions Roosevelt made. In the spring, the president was "reserved, withdrawn," while, by the

end of July, he "could see the whole picture . . . He was forceful, impatient with delay, pressing upon events" (p. 145). From that point to late September, Roosevelt transformed U.S. policy from benevolent neutrality to belligerency and risk of war. "The central dynamic of his policies was the conviction that the survival of the Soviet Union was essential for the defeat of Germany and that the defeat of Germany was essential for American security" (p. 179). Presumably what Roosevelt saw at the end of July was not that Germany had to be defeated but that German involvement in the Soviet Union gave the United States a way to bring about that German defeat. In effect, this was a strategic estimate on Roosevelt's part.

Heinrichs analyzes the strategic and sometimes the tactical issues with which the president and his advisers grappled but does not consider grand strategy other than that which existed in a war plan. He does not consider the extent to which Roosevelt saw the defeat of Germany as essential because of military or long-term economic concerns. Heinrichs argues that Roosevelt was not prepared to risk war in the Atlantic until he had hammered out an agreement with Winston Churchill about the postwar world. But what kind of world Roosevelt wanted is not examined. The focus is relentlessly on the immediate situations in which Roosevelt made decisions.

Although his book is on Roosevelt, Heinrichs is aware that the management of policy is influenced by bureaucracy, and he describes fights in the navy and between the navy and the army. But his over-the-shoulder approach to understanding Roosevelt means that we see the president making a decision on which branch got the few tanks that all wanted and lecturing the cabinet to get war supplies to Russia, but we lose sight of such issues once the president has turned his attention elsewhere. It is just when the president turns away that the "faceless bureaucracy" is free to act in accordance with its own conception of vital national interests, conceptions that can be quite different from those held by the president.

Furthermore, there are some areas of Roosevelt's leadership that Heinrichs does not address. The critical importance of mobilizing the economy to full war production is apparent from the chronic shortage of war supplies for the allies, but Heinrichs does not consider Roosevelt's leadership in this area. Similarly, a tentative public and an uneasy Congress caused the president to move more slowly than he otherwise would have, but the author does not address Roosevelt's leadership as a molder of public opinion or manipulator of Congress.

When discussing the State Department, Heinrichs concludes that the president was in control of diplomacy, even though he was content to turn the details of diplomatic discussions over to Cordell Hull and his associates. Thus, the dynamics of decision making within the department is less important than what was said and proposed. Ultimately, Heinrichs argues, the course of foreign policy rested in the hands of the

president who had a very clear idea of what he was about. There is nothing polemical in this study. At first glance it appears virtually free of judgment. But Heinrichs gradually builds a picture of a president who made the right decisions, given the military and political constraints under which he operated.

Some historians, myself included, who have put greater emphasis on grand strategic and bureaucratic considerations than does Heinrichs will continue to disagree with several of his conclusions. But we will also have to recognize that, in this book, Heinrichs has set a new standard for integrating strategic and operational considerations into the study of diplomacy, and we will never be able to write our diplomatic histories the same way again.

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LEON V. SIGAL. *Fighting to a Finish: The Politics of War Termination in the United States and Japan, 1945*. (Cornell Studies in Security Affairs.) Ithaca: Cornell University Press. 1988. Pp. xii, 335. \$39.95.

This is a wise, if somewhat overlong and wordy, study of a profoundly important subject: how states extricate themselves from war once victory and defeat become inevitable. Leon V. Sigal notes that most standard accounts (whether "orthodox" or "revisionist") of Japanese-American relations in 1945 argue that the two nations acted in rational ways, which were internally consistent and oriented toward external relations, to terminate the fighting. Surely, matters of national pride, bureaucratic rivalry, the emerging cold war, and a host of other factors complicated surrender. But, overall, students of the process see a series of "rational choice" calculations underlying the politics of war termination. Sigal rejects this explanatory model as factually wrong in the case of Japan's surrender and conceptually deficient as a general model of how states negotiate the end of hostilities. Instead, the author employs two alternative approaches, both of which "disaggregate the state into its constituent organizations and groups" (p. x). The "organizational-process approach" emphasizes the routine behavior of government organizations and their resistance to change, while the "internal-politics approach" stresses the competing interests of bureaus within the government as well as "conflicts among publics outside and suggests the difficulty of getting agreement within as well as between governments to end a war" (p. x).

Using a wide range of monographs, published documents, and some archival sources, Sigal weaves a powerful argument to test his model. By the beginning of 1945, politicians, bureaucrats, and the armed services in both Tokyo and Washington had little doubt about the ultimate outcome of the war. All understood that the United States would "win" and Japan would "lose." But this fact only made the chronic bureaucratic

fighting within each capital more intense. In America, each armed service branch had to prove that it, not a rival service, had won the war. To do otherwise would sacrifice advantage in the postwar competition for budgets and mission. Similarly, politicians believed that they could not risk being called "soft" on Japan, thus precluding a wide range of approaches toward the enemy. In aggregate, for largely domestic reasons, the United States sought to "shock" Japan into unconditional surrender at the very time Japanese leaders needed reassurances to get them to transcend their own internal disputes.

The situation in Tokyo bore a striking resemblance to that in Washington. Civilians, members of the royal family, army and navy leaders, and others all sought to pass blame for defeat on to their rivals. The military also convinced itself that "softer" peace terms could be obtained only by fighting a bloody last-ditch battle on the beaches of Japan or bluffing the Americans (through refusal to negotiate) about its determination to do so. Much of this posturing, Sigal shows, involved internal Japanese political maneuvering.

For a variety of reasons, including access to a richer secondary literature, the book is stronger on the American side. The important studies by John Dower (on the role of Yoshida Shigeru) and Waldo Heinrichs (on Joseph Grew) do not seem to have been consulted. And any analysis of internal Japanese factionalism is incomplete without a discussion of the role played by the influential *zaibatsu*, or monopolies.

Absurd and tragic ironies abound throughout the text. The author portrays the army air forces as mounting immense raids for which no real targets any longer existed in the final hours of the war, after Japan had accepted all of the U.S. terms. The sole purpose, it seems, was to show that the strategic bombing force could successfully fly one thousand planes in a raid.

The atomic bomb also presented some untoward problems of a strategic, if not moral, nature. (In some ways, Sigal's discussion about the politics of using the atomic bomb—a subject on which he has written previously—constitutes the most original research in the book.) American air, naval, and army commanders all worried that it might be too efficient a weapon, thus eliminating their calls for large postwar budgets. This concern caused a certain coolness among senior military leaders toward employment of the bomb. Still, Sigal confirms the findings of Martin Sherwin, Gregg Herken, and Michael Sherry that neither atomic bombs nor conventional strategic bombing "forced" Japan to surrender. The war ended when the hope of Soviet mediation faded and political factions in both nations mustered the will to stop slaughter that no longer had any purpose. This required reassurances and concessions on both sides. Nevertheless, in official histories of the war, the U.S. Army claimed the threat of invasion led to surrender, the army air forces credited strategic bombing for the victory, and the navy attributed success to blockade.

Sigal concludes the book with a long exposition in

which he applies his model to the potential problem of nuclear war termination. Although this section of the study makes some interesting points (such as the absurdity of "decapitating" the Soviet leadership, just as the Americans planned to use the atomic bomb against Tokyo in a final blow), it is less satisfying than the rest of the study. More telling is Sigal's evidence that the military services are woefully unprepared to conduct the delicate political negotiations that must precede war termination.

MICHAEL SCHALLER
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ROBERTO G. RABEL. *Between East and West: Trieste, the United States, and the Cold War, 1941–1954*. (Duke University Center for International Studies Publication.) Durham, N.C.: Duke University Press. 1988. Pp. xvii, 222. \$32.50.

In this finely crafted work, Roberto G. Rabel provides the most detailed and sophisticated analysis to date of U.S. policy toward the problem of Trieste after World War II and its significance to policy makers during the development of the cold war in Europe. Rabel traces U.S. policy toward the Italo-Yugoslav border conflict represented by Trieste from the beginning of World War II through the crisis of May 1945 and then through the early cold war years to the Stalin-Tito split and the "moderate" (p. 167) U.S. success in the settlement of 1954.

This book is part of a growing body of monographic studies on specific areas of conflict between the former allies and the interaction of individual policy makers. Rabel sees the Harry S. Truman administration's policy as representing "a microcosm of the unfolding Cold War" (p. ix), and he places his study in the framework of the postrevisionists and depolarization interpretations of the cold war most closely associated with John L. Gaddis (*The United States and the Origins of the Cold War* [1972]) and Fraser J. Harbutt (*The Iron Curtain* [1986]). He finds that the Truman administration, in responding to the Yugoslav occupation of Trieste in May 1945, "anticipated in certain ways . . . the basic Cold War strategy: containment" (p. 53). But, while arguing that the "administration's response to the Trieste crisis [was] an early step on the road to containment" (p. 53), Rabel maintains his main emphasis and focus on the specific problem and considers larger questions only when they impinge on policy decisions.

The close focus on policy making concerning Trieste provides both the many strengths and the few weaknesses of the book. Rabel is deft in handling the different issues that arose, and his description of the development of Truman's policy in May 1945 is superb. In the critical chapter on the May crisis, Rabel describes how State Department officials, particularly Joseph Grew, were able to cast the crisis in terms of Soviet expansion, depicting Tito as serving merely as a tool of Stalin. Presenting what the author labels a

"nascent 'Cold War consciousness,'" (p. 70), Grew stressed that "Russia was undoubtedly behind Tito's move" (p. 67) in occupying Trieste and argued that another policy of appeasement could not be adopted. As Rabel concludes, "The president required no further persuasion" and decided to demand that Yugoslavia leave or the United States would "throw them out" (p. 62). Yugoslavia, lacking Soviet support, quickly yielded to the threat.

Rabel, however, does not completely develop the implications of this conclusion and later argues that Truman's actions were part of a "general quid pro quo approach" (p. 64). Truman, Rabel asserts, recognized the limits of his ability to influence events in Eastern Europe and acted forcefully because he believed that Trieste was within the Western sphere of influence. Rabel concludes by arguing that Trieste may have served "to fashion a relatively flexible and restrained policy of 'containment' which realistically matched means with end" (p. 73) but was soon subordinated to a rigid containment policy. That Trieste quickly lost its primary role in the concerns of policy makers is made clear by Rabel's discussion of Trieste as a "pawn" in the cold war from May 1945 on and of the way it fit within the rigid cold war views developing in Washington.

Without a broader discussion by Rabel of Truman's views on Soviet expansion, particularly in relation to Poland, and his general conception of U.S. policy goals, it is difficult to discern whether Rabel indeed sees in U.S. policy toward Trieste an early step on the road to containment or a policy that was actually different from the containment doctrine, as he later suggests. The process by which Truman's thinking evolved is a difficult and critical question and one that underlies much of the controversy in cold war historiography. Rabel agrees with Gaddis that Truman wavered in the early months he was in office. Yet Rabel clearly demonstrates how the later logic associated with the cold war persuaded Truman to quick actions in Trieste.

These questions notwithstanding, Rabel has provided a significant contribution to the understanding of the cold war. In addition to discussing the immediate postwar move by State Department officials to explain events in Europe as all related to Soviet expansion, Rabel shows the difficulties often encountered in making policy with Great Britain and Italy, the usefulness of the Trieste "pawn" in the cold war for helping to maintain Italy as a stable member of the Western alliance and as a symbol of the cold war, and the propaganda value for the United States as the champion of liberal principles internationally. His discussion of the use and manipulation of Trieste in the Italian elections of 1948, although that episode is well known, adds to our understanding of U.S.-Italian policy and shows how foreign policy became trapped in rhetoric that soon, given the Stalin-Tito split, became a hindrance.

DAVID F. SCHMITZ
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THOMAS G. PATERSON. *Meeting the Communist Threat: Truman to Reagan*. New York: Oxford University Press. 1988. Pp. xiv, 317. \$24.95.

This volume consists of fourteen essays written by Thomas G. Paterson between 1969 and 1988 on various aspects of the cold war. As the title suggests, the basic theme of this collection is the author's contention that throughout the cold war the United States overreacted to the danger posed by the Soviet Union. The first eight essays, all relating to the development of the policy of containment under Harry S. Truman, stick closely to this argument, but the remaining ones, except for a new, perceptive view of the way Dwight D. Eisenhower and John Foster Dulles magnified the Soviet threat in the Middle East in the 1950s, lack any consistent focus, ranging as they do from the way the Vietnam War legitimized earlier isolationism to a survey of congressional oversight of covert activities. Although several of these essays offer useful insights, especially the one on the inability of Congress to monitor the Central Intelligence Agency effectively, they do not advance the author's central thesis.

The heart of Paterson's argument comes through in his essay on George Kennan, the disillusioned architect of the containment policy. Kennan's primary weakness, according to the author, was his insistence on putting great weight on communist ideology and thereby viewing the Soviet leaders as acting out of a Marxist desire to spread communism throughout the world. By accepting Kennan's analysis, Paterson argues, American leaders made the mistake when dealing with the Russians of "confusing goals with actual behavior" (p. 47). In the author's view, Soviet policy was the result not of ideological conviction but of pragmatic responses to expansionist moves by the United States. Paterson faults Kennan for his failure to realize that what Americans perceived as aggressive Kremlin behavior was in reality only a response to provocative American policies that stemmed from an exaggerated view of the Soviet threat. Thus, in Paterson's view, the United States was the primary culprit, bent on using its power to threaten legitimate Soviet interests.

The difficulty with this ethnocentric view of history is that it makes the Soviet Union the creature of misguided U.S. foreign policy. By denying any ideological component in Soviet policy, refusing to see the actions of the USSR as springing from internal needs and sources, Paterson rules out the possibility that the Soviets acted as they did for their own reasons, out of their own history and beliefs. In portraying the United States as the prime mover in the cold war, with the Soviet Union reduced to the role of simply responding to American initiatives, he offers a very one-sided view of the historical process.

Despite this flaw, Paterson's essays do provide some convincing examples of the way the United States overreacted to the Soviet challenge. In particular, he offers a stimulating analysis of the Truman administration's manipulation of public opinion on foreign policy

issues, an incisive critique of John F. Kennedy's crisis diplomacy, and a thoughtful rebuttal to the Vietnam revisionists, pointing out how difficult and costly it would have been if the United States had followed their advice and unleashed its full military power to achieve a victory in Vietnam.

The greatest value of this collection is to provide in one place the various criticisms of U.S. cold war policy that Paterson has put forward over the past two decades. It is to his credit that there is a high degree of consistency in his arguments, as well as an admirable restraint. If he thinks that the United States often harmed its own interests by exaggerating and overreacting to the Soviet threat, he does not deny the existence of some degree of danger nor accuse U.S. policy makers of acting out of evil motives. He argues instead, with considerable effectiveness, that they were often misguided and that a more sober, limited, and thoughtful response to Soviet challenges would have better served the national welfare in a dangerous world.

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ANTHONY CLARK AREND. *Pursuing a Just and Durable Peace: John Foster Dulles and International Organization*. (Contributions in Political Science, number 212.) New York: Greenwood. 1988. Pp. x, 243. \$39.95.

This work surveys John Foster Dulles's thoughts on the League of Nations, the United Nations (UN), regional organizations such as the Organization of American States (OAS), and international organization generally. Arranged chronologically by Dulles's decades at the bar, his years at the UN, and his tenure as secretary of state, it reveals Dulles's views on peaceful change, collective security, international law and courts, economic issues, and the role of international organizations in U.S. foreign policy.

Anthony Clark Arend bases his study on wide use of Dulles's published writings, on many but by no means all of the best secondary works, and on fairly good use of the Dulles papers. He provides at least some useful insights into every aspect of Dulles's views. Prior to World War II those included ruminations on the causes of war, how processes of peaceful change might prevent future wars, the nature of the state, the limits to ethical behavior, the nature of and limits to international law, how international law might develop toward greater usefulness, arms limitation, and "functionalism" (of which the Common Market is the best current example) as the most promising route toward viable international organization.

Dulles's role in the UN began with his helping to shape the charter at San Francisco in 1945 (especially regarding domestic versus international jurisdiction and the role of regional arrangements) and continued with his advocacy of U.S. membership. He granted the

charter's imperfection but saw it as merely a first step toward world peace, with its "greatest potential," as Arend puts it, "in the economic and social activities provided for in the charter" (p. 120). Those might have led, so Dulles thought, to the desired functionalism. It was as a U.S. representative to the UN that Dulles became interested in his country's image abroad, claiming that it "needed to demonstrate to the rest of the world its capacity to act decisively" (p. 135). Perhaps he believed this because of his changing view of the USSR. Before World War II he had not singled out that nation as a threat, but he now noted a Soviet "campaign of obstruction" at the UN as well as direct or indirect aggression, for example, in Greece and Korea.

The chapters on the UN serve as a kind of bridge to Dulles as secretary of state. As secretary he obviously concentrated on U.S. foreign policy; he continued to pay heed to the UN and the OAS; and he did, of course, create the Southeast Asia Treaty Organization. Under the rubric of "collective security," Arend recounts Dulles's use or disregard of such organizations in dealing with Guatemala in 1954, Indochina, Quemoy and Matsu, the Suez crisis, and the revolt in Hungary in 1956. Without admitting it as such, he tells of Dulles's surrender to right-wing senators on such issues as human rights.

This book is a useful summary of Dulles's thoughts and some of his deeds. It is largely well researched, but, because Arend ignores most of the political and diplomatic context, it is often rather abstract. Arend's prose is clear though pedestrian; there are very few errors of any kind; the footnotes are useful; but the index is woefully incomplete.

The major problem in the book is the divorcing of Dulles's thought on international organization from the context of his career and the political world in which he worked as well as his knowledge (or, in some instances, appalling ignorance) of global affairs. Arend shows good sense in relying heavily for his "appraisal" of Dulles on Ronald Pruessen's first volume of his projected two-volume biography, *John Foster Dulles: The Road to Power* (1982). When Pruessen completes his study, we will have what we really need on a significant figure.

ROBERT W. SELLEN
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H. W. BRANDS, JR. *Cold Warriors: Eisenhower's Generation and American Foreign Policy*. (Contemporary American History Series.) New York: Columbia University Press. 1988. Pp. xii, 252. \$30.00.

This book is a collection of biographical studies of leading figures in the Dwight D. Eisenhower administration's foreign policy apparatus. H. W. Brands, Jr., sets eight subjects, apart from Eisenhower himself, within three distinct categories. The first, the "inner circle," includes Secretary of State John Foster Dulles, Milton Eisenhower, the president's brother and prin-

cial confidant, and, to complete a remarkable pattern in fraternity, Allen Dulles, whose directorship of the Central Intelligence Agency, Brands argues, gave him a central position in the decision-making process. The second group, called the "lieutenants," consists of two men Eisenhower is said to have valued mainly for their executive and analytical talents in times of crisis: General Walter Bedell Smith, undersecretary of state in 1953–54, and Robert Murphy, the veteran diplomatic troubleshooter. Finally, a category called "agitators and agents" covers three officials who pressed particular interests: C. D. Jackson, the so-called psychological warfare specialist; Harold Stassen, the champion of disarmament policy; and United Nations ambassador Henry Cabot Lodge, who functioned as a kind of interpreter of and conduit to the emerging countries.

Eisenhower and his associates, Brands suggests, constituted a distinct generation of leadership. Unlike the principal figures of the Harry S. Truman era, many of whom were prominent before 1941 or became so only after World War II ended, most of the next administration's leaders came to the fore during the Second World War itself. The war stimulated in them a powerful but narrow ideological outlook that, together with what Brands calls their "unreflective" character (p. 204), led them to make an easy transference from Nazi Germany to Soviet Russia and to view the Soviet Union as simply the most recent embodiment of a continuing global totalitarian threat. This rigidity might have led to disaster as, Brands argues, it did when the "junior officers" of the John Kennedy and Lyndon Johnson administrations took over. Here, however, the experience of positions of high responsibility in wartime was salutary, for experience "indicated the limits of the ideology as a blueprint for policy" (p. 211)—hence, that strange combination of violent rhetoric and restrained action that characterized U.S. policy in those years. The Eisenhower people, partly from conviction and partly in deference to a shrill Republican right wing, talked tough. But, despite the proclamations of "brinkmanship" and "massive retaliation," they carefully avoided situations such as those in Indochina in 1954, in the Formosa Straits in 1955, and in Eastern Europe in 1956 that might have led to all-out war.

Much of Brands's analysis is persuasive. Inevitably, perhaps, given the unsettled character of professional opinion, a degree of ambiguity exists on the topical question of the Eisenhower administration's essential character. Brands, though often critical, reflects revisionist thought in portraying Eisenhower as effectively in control and Dulles as a sophisticated statesman. Much of the evidence, however, suggests that the liberal apparitions—Eisenhower the chronic procrastinator who ignored good advice and let problems fester and Dulles the clumsily dangerous moralist—cannot yet be laid to rest. Thus, Milton Eisenhower's continual pleas for more economic aid to Latin America as a guard against communism languished until the coming of Fidel Castro forced some action. A similar rejection

greeted Bedell Smith's repeated urging in 1954 that the United States should cut its losses in Indochina and accept wholeheartedly the decisions of the Geneva Conference. Stassen's persistent campaign for some sort of nuclear test ban also made little headway until 1958.

Certain problems of balance and coherence qualify the value of the book. The overall cold war context is thinly drawn. There is very little sustained treatment of European issues or of relations with the Soviet Union. The names Adenauer and Malenkov do not appear in the index; neither does Hungary. Nor, to pinpoint another absent dimension, is Charles Wilson listed there. Each subject receives a chapter of roughly equal length, from Dulles to the marginal Jackson, with Stassen, on top at last, receiving the longest. This well-written study is, nonetheless, a worthwhile, often stimulating contribution to a field undergoing very close scrutiny.

FRASER HARBUTT
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GLENN T. SEABORG. *Stemming the Tide: Arms Control in the Johnson Years*. Assisted by BENJAMIN S. LOEB. Lexington, Mass.: Lexington of D.C. Heath. 1987. Pp. xxi, 495. \$24.95.

Even as the Vietnam War and the war on poverty were overwhelming public attention during the presidency of Lyndon B. Johnson, the effort to bring atomic arms under control was going forward. If the task went by fits and starts, it was not for want of determined effort. A major figure on the American side of the undertaking was Glenn T. Seaborg, a Nobel laureate in chemistry, who for ten years beginning in 1961 had the honor and burden of being chairman of the Atomic Energy Commission. Drawing on his personal diaries and records, Seaborg offers a rare and detailed view of the process of conducting nuclear diplomacy with the Soviet Union.

No brief review can do justice to the intricacies and sensitivity of the nuclear arms control discussions. Nor can any words describe the patience and attentiveness to technical detail that they required. What stands out in the reader's mind, however, is that the larger public cannot have informed opinions on the subject. When one man with a musket was as good as any other man with a musket, disarmament—like the waging of war itself—was a subject for any citizen to probe and understand. Now the citizenry must rely on its political chiefs to choose experts, wise ones. But what of the chiefs themselves? Seaborg draws some conclusions: "Significant arms control achievements can be brought about only when the president takes a personal and an affirmative interest. . . . When President Johnson held himself aloof from the controversies generated by the differing points of view in his administration, the decision-making process . . . often seemed disorderly and unproductive" (pp. 449–50).

Because Johnson was omnipresent in the affairs of his administration, his aloofness in the matter of nuclear arms control may have gone beyond simple indifference or heedlessness. Heads of government agencies always complain that nobody is listening to them, sometimes not even their own bureaucracies. It is reasonable to say that Johnson understood better than his operatives that the wheel of history had not yet turned to where he could press for what was required. Johnson was not a close student of history, but he knew well that the harsh words spoken of each other by the Soviet and American leaders were part of the smoke screen of international relations.

One of the most illuminating portions of Seaborg's book deals with Plowshare, the program for the peaceful use of nuclear explosives in massive earth-moving projects, such as the creation of new canals and harbors. Seaborg writes, "The administration's uncertainty and internal conflicts about Plowshare might have been resolved more readily by the exercise of presidential leadership. But President Johnson was preoccupied elsewhere" (p. 325). If Johnson was slow to be convinced that the hour had arrived to deal effectively with the Soviets on arms control, he may have concluded that, regardless of what the scientists told him, the time was not yet ripe to reshape the earth.

HENRY F. GRAFF
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WESLEY T. WOOLEY. *Alternatives to Anarchy: American Supranationalism since World War II*. Bloomington: Indiana University Press. 1988. Pp. xi, 244. \$25.00.

Human welfare and perhaps even the survival of life on earth demand a system of effective supranational management, Wesley T. Wooley concludes in his chronicle of the efforts of elite American reformers to create such a system for the postwar world. The nation-state, itself an invention in the timeless search for order, can no longer cope. "Apocalypse is now a long-running serial," Susan Sontag recently noted (*New York Review of Books*, October 27, 1988). "Not 'Apocalypse Now' but 'Apocalypse From Now On.'" For nearly forty years, the world has endured the cold war threat of a nuclear exchange between the two superpowers; nuclear accidents at Three Mile Island, Chernobyl, and elsewhere; and nuclear proliferation, with the accompanying specter of terrorist access to plutonium. Meanwhile, in the new age of frontier competition on the high seas and in outer space, industrial and other pollutants have led to the apparent depletion of the ozone layer and the impending death of oceans, lakes, and forests from acid rain. Third World debt and population growth continue unchecked, while a poorly understood virus, AIDS, exacerbates global problems of race and class with each new case. Clearly, something needs to be done.

Taking the endless apocalypse and the near obsolescence of the nation-state system as starting points,

Wooley focuses on the efforts of such tough-minded global managers as George W. Ball, Zbigniew Brzezinski, Will Clayton, Cord Meyer, Jr., and others to make things right again. Wooley begins with the rise of the world government movement in 1945 and its fall in 1951 amid charges of subversion from the McCarthyites on the Right and the American communists and other Stalinist propagandists on the Left who did not share Lenin's view of nationalism as something peculiar to the economic epoch of capitalism. He next discusses the Atlantic movement of 1949-59, with its vision of a union of freedom-loving states banding together to stop the Soviet advance. Supra-cold warriors in the United States had room in their union for every Western imperial nation and even South Africa but not for 77 percent of the world's population. Finally, Wooley examines the global functionalist (or functional integrationist) movement, which came to center stage in the late 1960s.

In all cases, Wooley notes the scaling back of vision and goals from world government to a white Atlantic union to a functionalist approach to particular problems, arguing that public apathy, resilient nationalistic impulses, and the vagueness of supranationalism itself contributed to the failure of the one-worlders. Emphasizing fear as the basic human motivator, Wooley explains whatever success the world government, Atlantic unionist, and functional integrationist movements had by reference, respectively, to the bomb, the prospects of the wrong side winning the cold war, and the inevitable damage to the human environment by new and rapidly developing technologies. Ultimately, Wooley concludes that we are still waiting for a crisis big enough to bring the world to its senses.

This is a thoughtful book, by and large, with Wooley recognizing the paradoxical nationalism of the U.S. effort to create new global institutions of law and order patterned after existing U.S. institutions. The author fails to come to terms with that paradox in his conclusion about "an enduring commitment to new systems of world order" being dependent "on some dramatic failure of the old" (p. 207). Whether Wooley's characters, "well-educated, solid citizens deeply rooted in establishment culture" and prepared to rely on something more than "individual moral conversion or international good will" (p. 201), would lead the world to a more democratic order if given the chance is open to debate. For all the risks inherent in continuing the nation-state system in a shrinking and fragile world, the doctrine of national self-determination, without the Wilsonian baggage of a missionary United States in search of moral and economic preeminence, remains a more democratic vehicle for change than submission of Third World peoples to the new global managers. There is also something troubling about a prescription that relies on crisis to start forward motion to an ultimate good. Wooley realizes that a new order built on "some dramatic failure of the old" would be just as likely to move toward *supernationalism* and global authoritarianism. He leaves a hole in his book on that

issue. He also leaves a chronological hole from 1959, when the Atlantic unionist movement faded, to 1968, when the functional integrationist movement began.

Otherwise, this is a fine book, worthy of the attention of anyone who teaches the history of U.S. politics and diplomacy.

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KENNETH A. RODMAN. *Sanctity versus Sovereignty: The United States and the Nationalization of Natural Resource Investments*. (The Political Economy of International Change.) New York: Columbia University Press. 1988. Pp. xvii, 403. \$45.00.

Kenneth A. Rodman, a political scientist, has written a book that historians should find both intriguing and annoying. It is intriguing because the author is more interested in grouping events into discrete periods and in testing decision-making models than most historians are. Consequently, the book should force historians to think systematically about these issues. But it also is annoying because he uses a great deal of jargon that often has the effect, presumably unintended, of making things appear more complicated than they are.

Rodman concentrates on two interrelated issues. One is a change in the American attitude toward the nationalization of overseas natural resource investments. To chart the switch, he divides the analysis into three periods. From 1937–68, U.S. officials did everything possible to forestall expropriation and punish those few governments that dared nationalize American property. Things began to change between 1968 and 1974. A rapid increase in the number of nationalizations combined with dramatic changes in the international economic system made it impossible for the United States to continue the earlier policy. Since 1974 we have been in the third period, one in which Americans have become rather indifferent to the issue of nationalization.

The author identifies several reasons that caused U.S. attitudes to change. One is that government officials worried that disciplinary action might force an expropriating nation into the Soviet bloc. Another is that most governments that have nationalized foreign-owned properties have had to sell their raw materials in the world market. Moreover, they have been dependent on outside sources for capital, technology, and marketing. Thus, heads of multinational corporations have found that they can sign contracts that guarantee access to vital sources of raw materials, always their primary objective. Rodman places considerable emphasis on the extent to which the realities of the world market have worked to the benefit of multinationals. Finally, he notes that corporate executives discovered that relinquishing formal ownership to the host country enhanced corporate security by giving local govern-

ments a major stake in maintaining a steady flow of goods.

The second area of Rodman's concern is the foreign policy decision-making process. He has examined a large number of case studies over approximately forty years (1937–79) to see whether state officials are immune to corporate pressures, whether they function essentially as surrogates for multinational corporations, or whether the bureaucratic politics thesis makes sense. After trudging through the material, he ends on a quizzical note. Although able to dismiss the bureaucratic politics model, he is unable to measure the extent to which the corporate community has a privileged position that allows it to constrain the state. Nonetheless, he does think that business has a "privileged position within the minds of American decision makers" (p. 340).

The main weakness of the book is that the author tends to represent the world market, multinational corporations, the U.S. government, and the governments of less developed nations as independent, nearly free-floating, objects. What he does not see is that multinational corporations are capitalist organizations and that the world market is a capitalist market that exists because of actions by class-directed institutions, ones that are both public and private and that are located both in the United States and in the host nations. Because the international system is based on exploitative relations of production that privilege certain classes and class factions, particularly those that control multinational corporations, it should be identified as an imperialist system.

Without intending to do so, the author has described how the contradictions that exist within the imperialist world order of late capitalism have forced changes in the U.S. stance toward the nationalization of natural resource investments. Because the exploitative production relations remain intact, however, and because they are held in place in part by actions of the U.S. government, the system still is an imperialist one, and the U.S. state continues to function as an imperialist state. These are critically significant points that Rodman's interpretive framework cannot detect.

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CANADA

MARJORIE GRIFFIN COHEN. *Women's Work, Markets, and Economic Development in Nineteenth-Century Ontario*. (The State and Economic Life, number 11.) Buffalo, N.Y.: University of Toronto Press. 1988. Pp. x, 258. Cloth \$35.00, paper \$14.95.

The staples thesis, a congeries of ruminations about the way in which reliance on export markets for natural resource products shaped Canada's economy and society, is one of the polished old chestnuts of Canadian historiography. Until now, with the exception of the writings of Sylvia Van Kirk and Jennifer S. H. Brown

on women in the fur trade, the staples analysis has been of men and markets. In this excellent new book, Marjorie Griffin Cohen recasts the relationships among work, capital accumulation, gender, and the market in nineteenth-century Ontario so thoroughly and convincingly that the staples story ought never to be told in the same way again.

Cohen argues that the focus on the market in the analysis of nineteenth-century economic development neglects the household economy, the most prevalent mode of production of the time in terms of numbers of workers and level of production. The nonwage labor in subsistence production on the family farm is brought from the margins of conceptual consideration and made central to labor recruitment and capital accumulation in the staples industries and in the agricultural sector. The sexual division of labor was strict in pre-industrial production in Ontario, between men preponderantly in the market and women preponderantly outside it. Women's work in the subsistence sector, by providing for the family's basic needs, freed men to take up paid work in resource industries or to engage in agricultural production for the market. The female-dominated subsistence sector cushioned the volatile staple sectors. As a result, in the market part of the economy, wage bills were lower, relatively, and the rate of capital accumulation higher. On the family farm as well, nonmarket labor increased the funds available for investment in market-oriented activities. Textbook accounts have always emphasized that families were dependent on the seasonal work of men in resource and transportation industries for the cash with which to capitalize the farms. Cohen insists that this is only part of the tale and that timber barons, railway contractors, and fur buyers just as certainly built their fortunes by appropriating value created outside the market through family labor.

Although, as Cohen notes, the concept of family economy implies indivisibility of income and community of effort, the patterns in property ownership and inheritance are plain. The male head of the family alone owned and controlled the means of production in the farm family economy. "Ownership and control," she insists, "is as significant in the family economy as it is in more identifiably capitalist relationships" (p. 43). Her chapter on the changing conditions of women in the dairy industry tracks the patriarchal relations of production, showing how male property rights directed and diverted by gender the allocation of resources garnered by family labor, so that men's work on the farm became relatively more capitalized and efficient, while women's work by default retained rudimentary technology and relatively low yields. When market and government incentives were strong to invest in processes such as dairying, formerly a woman's domain, the gender ascriptions changed. Dairying, once mechanized and capitalized, became men's work.

Does this mean that, as Cohen argues, "wives and children were the proletariat of the family farm" (p. 44) and that whatever "antagonisms and contradic-

tions arose in the production process" were "less likely to be recognized as arising from different relations in production than from the ordinary *business* of getting along in a marriage [emphasis added]" (p. 116)? In revealing the persistent and systematic hierarchy in household power relations, Cohen has charted new territory in Canadian economic history. By naming this hierarchy with the diction of industrial class relations, she seems to be turning away from the opportunity her research has opened. I wish that she had defined the task of taking gender into account to entail reconceptualizing the process and apparatus by which power was exercised in a way that might have framed a new meaning rather than extending the compass of an old analogy. That said, this book is both startling and substantial and will have an enduring and beneficial impact on the writing of Canadian history.

JOY PARR
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C. IAN KYER AND JEROME E. BICKENBACH. *The Fiercest Debate: Cecil A. Wright, the Benchers, and Legal Education in Ontario, 1923-1957*. Toronto: University of Toronto Press, for the Osgoode Society. 1987. Pp. xii, 340. \$40.00.

Few issues have provoked as much controversy in the history of professional education as has the contest between scholarly learning and applied training. An important chapter in the saga is this study by C. Ian Kyer and Jerome E. Bickenbach. The book's title refers to the protracted and often bitter conflict between practicing lawyers and law teachers over the control and direction of legal education in Canada's largest province.

Founded on a permanent basis in 1889 by the Law Society of Upper Canada, Osgoode Hall Law School remained until 1957 the only institution in Ontario permitted by the legal profession to license lawyers. The University of Toronto established its own law department in 1930, but none of its graduates was allowed to practice without first attending Osgoode, a requirement that extended unnecessarily these students' professional training. This situation was one source of tension between the profession (represented by the law society) and the scholars. Another was the quality of legal education offered at Osgoode, which stressed the supremacy of practical office work over what many lawyers viewed as "cloistered," irrelevant classroom instruction. Unlike its counterparts in other provinces, the Law Society of Upper Canada insisted on maintaining a "concurrent" training approach in which students would attend lectures and work for lawyers on the same day. Osgoode students themselves deplored this system, although their complaints were usually ignored.

The professional academic torch was borne by Cecil A. Wright, a celebrated graduate of both Osgoode and

Harvard University. He taught at Osgoode and served as dean of the school before resigning amid a swirl of controversy to assume the deanship in 1949 of the University of Toronto Law School. Subsequent efforts to reach an accord between the profession and the universities finally bore fruit in 1957. Realizing that it could not cope alone with the expected increase in law school enrollments, the Law Society of Upper Canada agreed to recognize university law degrees in exchange for a training program that included, from the practitioners' perspective, a more favorable balance between classroom and articling requirements than universities had previously been prepared to consider. After decades of cold war, a rough peace was forged, which still constitutes the basis of legal education in Ontario today.

The story of this struggle is fascinating and well chronicled by Kyer and Bickenbach, although the book's contribution to the intellectual and organizational history of legal education is limited. The portrait of Wright is not consistent. He appears both as a missionary spreading the academic gospel of Harvard to the backwaters of Ontario and as a training enthusiast (not unlike the professionals he so long confronted) with only a minimal interest in legal theory and sophisticated education. The reader is thus left puzzling over his real motivations. In general, the authors seem too inclined to take the protagonists at their word without probing in more depth the competing professional, ideological, and territorial interests that lay behind the rhetoric of both practitioners and academics. Herein lies the potential for further study, which might build on the solid groundwork laid by Kyer and Bickenbach.

PAUL AXELROD
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PAUL VOISEY. *Vulcan: The Making of a Prairie Community*. (The Social History of Canada, number 43.) Buffalo, N.Y.: University of Toronto Press. 1988. Pp. x, 341. Cloth \$37.50, paper \$18.95.

Wheat farms and elevator towns were once actually and symbolically dominant in Canadian prairie life. Although nearly five million people now live in the three provinces of the prairies, fewer than 20 percent of them are in rural districts, and even the recent grain price and drought crises have not won sustained public attention. That popular neglect is only partly reflected in scholarly literature, but, despite valuable new works on agrarian politics and ethnic settlement, our picture of rural social relations has remained relatively unchanged over the last thirty years. This book redresses the balance.

Paul Voisey's revision of his doctoral dissertation has a distinctly North American air. He has read all of the secondary works on the American plains, and he has taken seriously the frontier and environmentalist inter-

pretations of that experience. His purpose is to test those two models of social explanation and their opposites—interpretations that emphasize the power of the metropolis and of cultural tradition—in the early history of one small district in south-central Alberta. The twenty-five-by-forty-mile block, midway between Calgary and Lethbridge, stretches on either side of a branch railway line; the largest of six villages on the line is Vulcan. The area was settled in the first decade of this century by farmers of British Canadian and American origin, mainly Protestants. Voisey assesses the community they created between 1900 and 1940 in thematic chapters on the settlement process, agricultural adaptations, and social institutions.

This book contains many valuable insights into farm and village life. It offers excellent discussions of small-town boosterism and of the absence within the district of class or other group-based conflicts. The analyses of cultivation techniques, crop selection, and farm size are consistently illuminating. In those sections Voisey is often tendentious, but he makes strong arguments. Readers should beware the underlying message, however, because Voisey is a thoroughgoing revisionist. The opening section of the book emphasizes that prairie pioneers accepted the prevailing "progressive" ethos of turn-of-the-century North America and were aggressive, even speculative, in their approach to life. Voisey is attacking, without saying so, the view that pioneers were remade by the frontier and sought to create ideal communities. His chapter on settlement discounts earlier scholars' sympathy for failed homesteaders and argues that many pioneers were simply gamblers in search of wealth. Similarly, the chapter on townsite development concentrates on the themes of capital gains and geographic mobility rather than on those of service and stability. Voisey rejects the usual platitudes about education and religion, arguing instead that the Vulcan area's one-room schools offered a poor excuse for learning and that its churches were irrelevant except as social conveners. In Voisey's view the newcomers to the Vulcan area were more dedicated to free enterprise than to cooperation; their wish to make money was stronger than their desire to pursue a vocation or enjoy a way of life; they were secular, not religious; pragmatic, not idealistic. Inherited cultural patterns delayed rather than enhanced their adaptation to the new environment, and the low population of the district was at least as important as metropolitan forces in shaping their society. That interpretation is guaranteed to irritate readers accustomed to the emphasis on the social gospel, social democracy, and social concern that prevails in prairie historical interpretations.

Voisey's discussion of frontier, metropolis, environment, and tradition ends rather lamely with an acknowledgment that all are influential factors and none is preeminent in shaping the Vulcan district. And his free enterprise version of prairie history faces a formidable adversary in the historical tradition established by V. C. Fowke and carried on by many contemporary

scholars. Nonetheless, it would be a shame if Voisey's book is viewed as cynical when it is simply—and gleefully—revisionist. Were prairie pioneers distinguished by materialism rather than by selflessness? Voisey says yes. He provides a dash of realism in his approach to rural life, and, if his account is less positive—dare one say less balanced?—than it might have been, it still has merit as a corrective to the dominant view in the historiography of prairie Canada.

GERALD FRIESEN
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DENIS SMITH. *Diplomacy of Fear: Canada and the Cold War, 1941–1948*. Buffalo, N.Y.: University of Toronto Press. 1988. Pp. x, 289. Cloth \$35.00, paper \$15.95.

The author of this book, Denis Smith, is also the author of a biography of Walter Gordon, a Canadian politician noted for his opposition to U.S. influences in Canadian economic life. That study, Smith writes, led him to conclude that "Canada's dependence upon the United States in the 1950s and 1960s" (p. ix) stemmed from events of the immediate postwar period, which he examines in this book. That conclusion, combined with the fact that Smith uses as one of his epigraphs a characteristically anti-American quotation from Donald Creighton, might lead the reader to write this book off as a mere polemic. It is, however, more than that. Although certainly intended as an argument for a specific political point of view, this study represents a formidable piece of research in archival sources and secondary works and is a useful addition to the historiography of the postwar period.

Canada's diplomatic establishment had been enlarged and strengthened during World War II, and, in the postwar years, the lately recruited diplomats (like those of Britain and the United States) engaged in active written discussion of the problem of relations with the Soviet Union. Much of Smith's book is taken up with analysis of that extended essay contest, in its national and international aspects. Smith also devotes much space to the policies of the Canadian prime minister, William Lyon Mackenzie King, who was in the final period of his long tenure of office. The elderly statesman does not emerge well from Smith's examination. Smith describes him as "pietistic, cautious, neurotic and ignorant of international politics" (p. 220). Actually, the author makes King, even from the Canadian nationalist point of view, rather worse than he was. The years 1946 and 1947 witnessed what amounted to an attempt by the United States to establish American military power on a large scale in the Canadian north. Months of discussion resulted in February 1947 in a new Canadian-American defense agreement that was full of saving clauses; in Smith's words, "the defense preparations which resulted in the north were minimal" (p. 179). King had quietly beaten back what, from a Gordonite perspective, was a dangerous American initiative. But all the prime minister

gets from Smith is a report of "a gigantic veil of euphemisms" (p. 178). The American military program, if carried out, would certainly have produced a hostile reaction from the Soviet Union.

The book ends with the King government, in its last days, approving in principle the idea of the North Atlantic Treaty Organization. That development was largely a product of the Communist coup of 1948 in Czechoslovakia. It was typical of King at this stage of his life that he was guided by London, accepting Clement Attlee's and Ernest Bevin's interpretation of events. Smith sees all of these policies as the regrettable result of diplomacy dictated by fear. His lugubrious comment is "the world still lives with its consequences" (p. 236).

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LATIN AMERICA

LINDA A. NEWSON. *Indian Survival in Colonial Nicaragua*. (The Civilization of the American Indian Series.) Norman: University of Oklahoma Press. 1987. Pp. xiv, 466. \$36.50.

Linda A. Newson complements her impressive *Cost of Conquest: Indian Decline in Honduras under Spanish Rule* (1986) with this equally fine study of population in adjacent Nicaragua from aboriginal times to the end of Spanish rule in 1821. Her study is based on printed materials and extensive research in manuscript records, no easy task because of their dispersal and loss. Where gaps in evidence exist, she points to them. Her assessments are sober and conservative; her use of geographical and ethnohistorical criteria, a model. In her approach as geographer and ethnohistorian, interest in numbers expands to include land and resources, forms of human occupation and exploitation, religion, and political and social structures—a broad scope.

She analyzes Nicaragua in terms of two basic cultural regions. The Pacific lowlands, in which she includes the Pacific coast of northern Costa Rica and the Nicoya peninsula, constituted the southernmost extension of Mesoamerican culture. Its people lived in dense settlement based on the traditional Mesoamerican staple of maize and were organized in chiefdoms with a civil and religious hierarchy. The far greater area of tropical forest draining to the Caribbean constituted an extension of South American culture. Its inhabitants practiced shifting cultivation based on root crops and perhaps plantains, supplemented by hunting and gathering. Organization was egalitarian, in small tribes, with little specialization.

Contact with Europeans was almost equally destructive in both zones. The Mesoamerican zone, densely settled and hierarchically organized, was conquered quickly and the Indians exploited through slaving, enforced tribute, and delivery of workers. In the course of centuries, as Spaniards and blacks settled among the Indians, mixed bloods became dominant.

Maize and other subsistence crops remained important, supplemented by cacao, cotton, indigo, tobacco, sugar, and livestock. Mining supplied some specie. The South American zone, entered initially for slaving and mining, was penetrated much more slowly; missions became a significant Spanish agency. Foreign interlopers, most of all English, entered the forests in search of logwood, mahogany, tortoise shell, and general trade. The English with black allies and fugitives settled on the coast, creating what Newson calls the Zambo-Mosquitos, a mixture of three races, adapting to Indian culture, hostile to the Spanish, and expanding westward. Both zones lost population through epidemics, slaving, warfare, and exploitation—in the Mesoamerican zone in the form of *encomienda* and forced labor, in the South American zone in the form of Spanish missions. The Mesoamerican zone lost population quickly, reaching nadir in the seventeenth century, when recovery started. In the South American zone, numbers declined much more slowly, but decline continued into the eighteenth century. Newson estimates the aboriginal population at eight hundred twenty-six thousand, of whom approximately 75 percent lived in the Mesoamerican zone. At nadir in both zones Indian numbers fell by over 90 percent. When recovery came, it was attended by the emergence of mixed bloods of Hispanic culture except among the Zambo-Mosquitos, mixed bloods also but linked to the English. Newson analyzes the processes systematically and in much detail.

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BERNARDO GARCÍA MARTÍNEZ. *Los Pueblos de la Sierra: El poder y el espacio entre los indios del norte de Puebla hasta 1700*. (Centro de Estudios Históricos.) Mexico City: El Colegio de México. 1987. Pp. 424.

Bernardo García Martínez has written an insightful and carefully documented study of the mountainous northern part of the Mexican state of Puebla from the preconquest period through the early eighteenth century. His principal concern is the spatial reorganization triggered by Spanish colonization and the resultant evolution of the indigenous community, or *altepetl* (plural, *altepeme*), into the late colonial *pueblo de indios*.

Corresponding to the jurisdiction of a local ruler, the preconquest *altepetl* operated without fixed geographical boundaries or a designated center. Sixteenth-century Spaniards had trouble reconciling these concepts of spatial organization with their own Renaissance notions of "civilized" community life, which emphasized the clear delineation of jurisdictional limits and the concentration of religious, political, and economic functions in well-defined centers. Although superficially respecting indigenous political units, Spanish priests and bureaucrats, in fact, irreversibly undermined the old order as they went about selecting

principal towns (*cabeceras*) to serve as administrative and ceremonial capitals under the new regime. Despite notable examples of successful Indian resistance, the forced resettlements (*congregaciones*) of the mid-sixteenth and early seventeenth centuries further altered the sierra's spatial organization. Moreover, during the seventeenth century, increasing numbers of subject communities separated from their *cabeceras*, forming new jurisdictional patterns even more remote from preconquest practice. By 1700 the appearance of what García Martínez calls "second generation pueblos," or independent communities not descended from original *altepeme*, signaled near completion of the shift from indigenous to Spanish modes of spatial organization. Community fragmentation continued through the eighteenth century, as some towns divided again to form "third generation pueblos."

Fully encouraged by the colonial government, this centrifugal process created political units of constantly diminishing power and size. As localized cults increasingly provided the only real basis for social cohesion, *cofradía* treasuries supplanted the impoverished *cajas de comunidad* as repositories of pueblo assets. As a result, progressively fewer local Indians coveted the office of *gobernador*; on occasion colonial officials even resorted to appointing outsiders recruited from a group of "professional *gobernadores*" who transferred from one post to another like any other colonial bureaucrat. García Martínez concludes that the Indians' acceptance of such governors further documents the deteriorating political significance of the pueblos. Meanwhile, growing numbers of non-Indian pueblo residents, mostly petty merchants and small farmers, abetted and profited from these changes. Disregarding the jurisdiction of local Indian governments, they steadily expanded their landholdings and created islands of "extraterritoriality" within the Indian communities.

García Martínez also describes the local operations of the *encomienda*, the *repartimientos* of labor and merchandise, early evangelization, and the marginalization of Indian caciques. In sum, he offers both richly detailed portraits of a region little studied by other historians and fresh observations on the profound transformations that swept Indian communities throughout central Mexico during the sixteenth and seventeenth centuries. His work deserves a serious reading by every colonialist.

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RICHARD J. SALVUCCI. *Textiles and Capitalism in Mexico: An Economic History of the Obrajes, 1539–1840*. Princeton: Princeton University Press. 1987. Pp. xiv, 249. \$40.00.

For some time the *obrajés*, or textile manufacturing shops, of colonial Mexico have been the object of quite frequent, if rather glancing, attention from historians.

Not until now, though, have they been observed en masse and over the whole span of their existence, from soon after the conquest until the immediate postindependence years.

In five succinct chapters, Richard J. Salvucci examines artisan cloth production in the *telares sueltos*, units smaller than *obrajes*; the *obrajes* themselves, their locations, and their processes; the business of operating an *obraje* and some of the businessmen who did so; the labor force producing textiles and the means by which it was secured; and long-term production trends by region. A brief epilogue sets the Mexican textile business in the context of colonial Spanish American cloth making as whole and offers some economic and political generalizations that the story of the *obraje* suggests.

It is indeed one of Salvucci's main purposes in this book to use the history of the *obraje* as a window on the operations of the entire economy of colonial Mexico. His fundamental contention is that the *obraje* was shaped, and its development controlled, by the nature of markets in New Spain, both the markets providing factors of production (especially capital and labor) and the market in which cloth was sold. Imperfections in those markets, he argues, limited the growth of textile production and especially hindered the technological advance of *obraje* manufacturing, so that, by the time of independence, Mexican cloth making was wholly at the mercy of the phenomenally rising productivity of the British textile mills. In the absence of a smoothly operating labor market, *obraje* operators used any political power they could deploy in order to obtain workers. The discussion of labor supply and conditions presented here will be of particular interest to many readers, given the unsavory reputation as a workplace that the *obraje* has acquired from earlier studies by historians. Some of that reputation is deserved. But the true picture has, as might be expected, far more complexity than the one that has been presented.

The general applicability to the colonial economy of the processes that Salvucci has identified remains to be tested. A brief look at, say, mining or sugar production, employing the same principles as those applied to the *obrajes*, would have been illuminating, but that would be perhaps an excessive demand to make. The fact that most of the information presented refers to the eighteenth century also gives one cause to wonder how well the conclusions may hold for the entire colonial era. But such doubts are far outweighed by the positive qualities of the book. The grounding in thorough archival research, the treatment of an entire industry, the systematic reference to economic theory, and the subtlety and flexibility of thought displayed throughout by the author, all make this study a remarkable addition to present knowledge. It is a book that will repay careful and meditative reading. And, if it is widely read in that way, it may well help to raise the standard of study of Spanish American colonial economic life.

PETER BAKEWELL
University of New Mexico

ALEX M. SARAGOZA. *The Monterrey Elite and the Mexican State, 1880–1940*. Austin: University of Texas Press. 1988. Pp. x, 258. \$30.00.

Alex M. Saragoza examines what is probably the central issue of modern Mexican political history: the struggle between regional elites and the central government. He does so through the study of one of the most powerful of these factions, the *Grupo Monterrey*, or *regiomontanos*, as the elite families came to be called. The *Grupo* was exceptional in Mexico in that it based its wealth on industrial enterprise. It was even more remarkable in that it survived the revolution (1910–20) and its aftermath (1920–34) and emerged even stronger than before. Saragoza maintains, furthermore, that the *Grupo Monterrey* almost single-handedly, through its fierce resistance to the revolutionary national government, moved the Mexican regime to the Right after 1938 and established the foundation for the post-1940 capitalist development of Mexico.

The Monterrey elite survived the revolution for two reasons. Members held no overt political power during the prerevolutionary era, operating instead through state bosses such as Santiago Vidaurri and Bernardo Reyes. As a result, the discontented middle class that led the revolution was more inclined to emulate these men than to overthrow them. In addition, the wealth of the *regiomontanos* was not land-based and therefore did not suffer the cycles of reform and redistribution that periodically devastated other elites.

Saragoza asserts that the strength of the Monterrey elite lay in extensive family ties that produced a general homogeneity of interests and timely new leadership. He also sees its power in its leaders' total belief in their own entrepreneurial abilities and the correctness of their vision of Mexican development. Conflict with the postrevolutionary regimes under Presidents Alvaro Obregón, Plutarco Elías Calles, and Lázaro Cárdenas arose, according to the author, because political expediency forced the national government to introduce labor reforms and because revolutionary politicians at the state level were corrupt and unreliable.

Saragoza focuses on the two major political struggles of the *regiomontanos* against postrevolutionary regimes. The first battle was in 1929, when the *regiomontanos* pushed hard to elect Aarón Sáenz president, and the second was in 1940, when they financed the presidential campaign of Juan Andreu Almazán. Both candidates lost. The *Grupo* ultimately won out, however, for after 1940 its vision of Mexican development prevailed. How did the Monterrey elite win? Saragoza maintains that it was a combination of three factors. The *Grupo* was too costly an adversary, so cohesive and stubborn were its leaders. Moreover, the leaders of the revolutionary regime were not fundamentally opposed to the Monterrey view but had acted on reform only as a political expedient. Finally, by 1938 the nation was worn out from reform. The Monterrey clans simply outlasted radical zeal.

The book is a fine example of regional and family

history. Saragoza's argument is clear, though his prose is sometimes inelegant. The main weakness is the author's decision not to describe in more detail the *Grupo's* economic power (size of its enterprises, employment, profits, and so forth). Thus, the reader can only guess how entrepreneurs running a nonessential industry (a brewery and glass works) could stand off the Mexican government.

MARK WASSERMAN
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JOHN MASON HART. *Revolutionary Mexico: The Coming and Process of the Mexican Revolution*. Berkeley and Los Angeles: University of California Press. 1987. Pp. xi, 478. \$35.00.

John Mason Hart's extensive study of the Mexican revolution is the best work to date on major elements of Mexican society that became revolutionary actors. In the first part of his work, he discusses the peasantry, the industrial and urban workers, what he calls the "pequeña burguesía" and the provincial elites, and investors and businessmen from the United States. He is particularly strong in supplying new material on the ties between Texas businessmen and various political and economic groups in northern Mexico, the area that was the major point of origin for the revolutionary movement of 1910. In addition, he includes an interesting comparative chapter in which he discusses elements of "global causation"—the search for natural resources, cheaper labor, and potential markets by American and European industry—and their effects on revolutionary situations in Iran, Russia, and China that occurred at about the same time as the Mexican revolution.

In his discussion of the revolution itself, however, he imposes rather too heavy a class analysis, based largely on the origins of the leaders of various revolutionary factions. In this effort he not only explicitly rejects the role of personality, an error in my opinion, but also fails to look closely at the composition of those factions and has at times strained his evidence to try to make one leader or another fit into his categories. Most revolutionary forces were highly fluid, and many local and regional leaders as well as many of the rank and file moved from one faction to the other quite readily as they perceived particular advantages. An important question, it seems to me, is why different individuals and groups perceived particular alliances to be in their interest—a question intimately linked with aspects of personality as well as military ability and success.

Hart has dealt with the question of advantageous alliances only in regard to the labor movement and shows quite clearly why this group supported the Constitutionalists, whom Hart characterizes as principally "pequeña burguesía" (pp. 14, 16), a term that is at best artificial in the Mexican situation and has the unfortunate effect of implying misleading similarities with European social structures. When the leaders of

the Casa del Obrero Mundial, the most important labor organization at the time, joined the Constitutionalists against the Villistas in February 1915, they were certainly adhering to the "likely winner" (p. 307). In addition, as Hart ably explains, they had little fellow feeling for the rural revolutionaries who followed Pancho Villa and particularly Emiliano Zapata, seeing them as too "primitive" (p. 305) or too Catholic and too unlikely to succeed either militarily or politically. An alliance with the Constitutionalists was the only alternative to staying out of the fray altogether and seemed to offer more rewards. It should be emphasized, however, that the participation of the Casa was of limited duration. Further, much of the Constitutionalist following was agrarian in nature. Thus, it seems likely that leadership and personality were more important than class in winning the Casa's support.

My major objection to this study is the importance that Hart gives to the activities of the U.S. government in the triumph of Constitutionalist forces over Villa and Zapata. He bases his assertions on new information about the large amounts of military supplies that U.S. troops left at Veracruz when they pulled out in November 1914. Hart asserts that the abandonment of these supplies, some of which had been there since before the fall of the Victoriano Huerta government, was a deliberate and successful effort by the U.S. government to turn the tide of war in favor of the Constitutionalists. That, I believe, remains to be proven. Convenience or ineptitude are also likely explanations, although, regardless of intention, the Constitutionalists were greatly aided. He further indicates that the U.S. arms embargo in the north made it impossible for Villa to obtain shipments from across the border. He exaggerates, however, the effectiveness of the embargo, and Villa was able to get supplies at a reasonable price and sometimes on credit until after his defeat by Álvaro Obregón at Celaya. American suppliers ceased to provide favorable terms to Villa when it became apparent that he was losing.

Hart also asserts that the Constitutionalist victories in the Bajío were the result not of better generalship but of "alien advisers" (p. 311) and the equipment left by the Americans. He offers no source for this assertion and no supporting evidence. The Ronald Reagan administration provided a great deal of both military hardware and advisers to Central America with little visible success, and it seems unlikely that such aid would have led as easily to a Constitutionalist victory as Hart indicates. If the Constitutionalists had not had internal support, the arms supplied would not have been sufficient, and there is no indication that American advisers were in any way major influences on the Constitutionalist military leadership. Further, Hart fails to mention that the Villa-Zapata alliance had crumbled by the time of those battles. Had Villa and Zapata continued to cooperate, Obregón might have been stopped before he got to the Bajío, or Zapata might have been able to mount a significant rearguard action and cut Obregón's lines of supply to Veracruz.

Despite my differences with Hart's interpretations of the "process" of the revolution, it is important to say that his work is stimulating and challenging and that he introduces new material on U.S. economic involvement in Mexico that is of enormous interest. Along with Alan Knight's recent two-volume work and the plethora of excellent new regional studies, this book adds significantly to a more sophisticated understanding of the Mexican revolution.

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DAVID BUSHNELL AND NEILL MACAULAY. *The Emergence of Latin America in the Nineteenth Century*. New York: Oxford University Press. 1988. Pp. viii, 335. Cloth \$32.50, paper \$11.95.

This book by veteran scholars David Bushnell and Neill Macaulay is definitely the first of its kind, a survey in a single volume of Latin American political history from independence (1821) to the 1880s. On that count alone it is a valuable contribution. Moreover, the authors weave their narrative with exceptional skill, the contents presented in a logical manner that is not dependent on mere country-by-country chapter divisions. By the end of the book, all of Latin America has been discussed to some degree, although some sections, such as that on Central America, suffer from the attempt to summarize too many events too briefly. Particular attention is paid to economic developments, constitutionalism, and church-state relations, and the rise of nineteenth-century Latin American liberalism is the overarching theme.

Persons acquainted with some of the main themes in the nineteenth century will find the book fascinating, because it points to common developments that affected all of the Latin American countries. The beginner, however, might have real difficulty in absorbing this material since even an abbreviated narrative of the complex political history of the nineteenth century is still dizzying. Furthermore, it is disappointing that the authors end the narrative in the 1880s, because many historians will argue that the liberal experiment was just then starting to show its ultimate consequences. There is only passing reference to the logical outcome of nineteenth-century liberalism in the period from 1880 to 1914. Why does the narrative not carry through with the fall of the empire and the rise of the republic in Brazil, the Porfiriato in Mexico, or the war of 1898 in Cuba?

One is left with the suspicion that the last decades of the century may inconveniently contradict the authors' clear preference for market forces and their generally approving nod toward the idea that there is such a thing as "progress." Bushnell and Macaulay include little or no discussion of the social costs of nineteenth-century liberalism, of the implications of ideas in the age of Social Darwinism, of the rise of U.S. hegemony, or of agro-industry. Instead, we are told that the critical

interpretations of André Gunder Frank and E. Bradford Burns, concerning dependency and progress respectively, are inadequate. It may be useful to reject conspiracy theories and class conflict as keys to interpretation, but the invisible hand works no better. Arguing that market forces and enlightened self-interest determined everything is not satisfying in the absence of discussion of big-power pressures, intellectual currents, and labor conditions. Although Bushnell and Macaulay reject dependency as an explanation, it is a distinctly "vulgarized" dependency concept that they are resisting, and their rejection of the concept of an elite as a single interest group on the grounds that not all of its members were born to the elite is not persuasive.

The underlying assumptions of the authors are problematic, no more so than in the brief concluding chapter reprising the neoclassical definitions and approaches that suffuse the work. A gratuitous comment linking debt peonage in the 1880s and the debt crisis of the 1980s crystallizes the authors' support for exported growth and open economies and is a shocking statement for a scholarly book. Might there not be perfectly valid reasons other than sheer perversity that explain why citizens of less developed countries would opt for nationalism and protectionism? Readers will have to decide whether Bushnell and Macaulay help focus the debate or mislead it.

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BILL ALBERT. *South America and the First World War: The Impact of the War on Brazil, Argentina, Peru, and Chile*. Assisted by PAUL HENDERSON. (Cambridge Latin American Studies, number 65.) New York: Cambridge University Press. 1988. Pp. x, 388. \$42.50.

This study by Bill Albert covers neither all of Latin America nor all dimensions of the "impact" of World War I, being concerned primarily with the economic effects and, secondarily, with their political implications. But it treats the questions of greatest theoretical interest for the region as a whole. Did the crisis of the developed industrial nations provide an opportunity (as André G. Frank, for one, has intimated) for dependent Latin America to embark on a more autonomous path of development? If so, why were the results seemingly so limited? If not, why not? Albert approaches these questions through a detailed look at the changes caused by the war in Latin American commerce and finance, the response of Latin America's manufacturing industry, and the social conflicts triggered or aggravated by those developments in four countries that serve as case studies. His answer to the first question is a qualified no on the ground that the very nature of Latin America's previous export-oriented development made the dominant groups ambivalent or even hostile to the possibility of an alternative project and that the existing pattern had favored the

creation of financial systems, transportation infrastructure, and so forth, with a built-in export bias. The wartime demand for particular Latin American commodities was still another factor working against any fundamental reorientation of the economy. Thus, although some increase in domestic manufacturing occurred, Albert tends to minimize its significance, and his treatment of most other seemingly positive developments is quite comparable. The "perfidiousness of the international economy" (p. 222)—before, during, and after the war—drastically limited Latin American options in every case.

Though largely based on secondary works, and for that reason admittedly uneven in coverage of particular countries and topics, this study does carefully take note of disagreements among scholars who have looked at the same problems before, including what he calls the "informative acrimony" (p. 202) that has surrounded discussion of wartime industrialization in Brazil. In general, the analysis is convincing. It is written from a "dependency" perspective, but this is not "vulgar" *dependentismo*. Albert produces concrete evidence to support his interpretations, and he recognizes the extent to which "dependency" was a symbiotic relationship voluntarily entered into by Latin America's political and economic leaders. It is mainly in dealing with labor unrest that his argument seems to outdistance his facts. He sees the strikes and increased militancy of the later war years and early postwar period as constituting a true watershed, whereas in other respects the war is shown to have been a severe crisis that reinforced (or hindered) trends already evident but did not represent a genuine turning point. The seemingly disproportionate "impact" on labor is not satisfactorily explained. On a number of other occasions, most notably in the chapter on finance, the sheer mass of detailed information overwhelms the reader. And the remarkably poor editing and proofreading by Cambridge University Press—presumably not all of the erratic placement of commas and the use of plural verb with singular subject or vice versa can be attributed to the vagaries of British English—are a distraction. But the work is important enough for the discerning reader to plow ahead—and with profit.

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KRISTIN HOFFMAN RUGGIERO. *And Here the World Ends: The Life of an Argentine Village*. Stanford: Stanford University Press. 1988. Pp. xvii, 226. \$32.50.

The author of this book first went to Argentina in 1976 to write a history of Colonia San Sebastiano, a village of approximately one thousand people in the province of Entre Ríos. What attracted Kristin Hoffman Ruggiero to this remote community is that San Sebastiano is still largely inhabited by descendants of Italian immigrants belonging to a little-known Protestant sect called the Waldenses. Despite her efforts, Hoffman Ruggiero was unable to find the necessary sources for the reconstruction of the colony's history and therefore decided to write a book about life in a small rural community.

The result is a detailed account of the villagers' daily activities through numerous vignettes intended to give an intimate picture of the lives of farmers, school children, housewives, and peons. The author describes Colonia San Sebastiano as an isolated, marginal community, "at the edge of the Western world" (p. xv). It is a village whose inhabitants live in harmony and adhere to the principle of avoiding confrontations. Although old-timers may recall past tensions between immigrants and native Argentines, all presently live in an almost idyllic world without social or political tensions despite poverty, inflation, and the presence of a military regime in the capital.

The picture drawn by Hoffman Ruggiero is far from convincing. In her examination of a country deeply divided by conflicts between Peronists and anti-Peronists, for example, we are not told the villagers' views on Peronism, and there is no discussion of how the community has been able to avoid the ideological debate that has torn Argentine society since the 1940s. Furthermore, the author contends that distance and isolation have tempered the effects of military rule on Colonia San Sebastiano. That may well be the case, although she mentions police barricades in the streets, military personnel patrolling picnics, and people discussing Argentina's bad image in the foreign press because of human rights violations and censorship (the name of one of the forbidden authors, Paulo Freire, is misspelled). But Hoffman Ruggiero fails to explain how the villagers lived before the military regime came to power and, more important, how individual members of the community perceived the changes brought about by military rule. If we are to believe her account, we need to know to what extent and in what ways the long arm of the junta contributed to the unusual "harmony" and "lack of confrontation" (pp. 23–24) that she found in Colonia San Sebastiano.

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Collected Essays

These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed.

RALPH MELVILLE *et al.*, editors. *Deutschland und Europa in der Neuzeit: Festschrift für Karl Otmar Freiherr von Aretin zum 65. Geburtstag*. In two volumes. (Veröffentlichungen des Instituts für Europäische Geschichte Mainz, Abteilung Universalgeschichte, number 134.) Stuttgart: Franz Steiner. 1988. Pp. xxii, 521; x, 526–1019. DM 198.

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sche Beschreibung. JOHAN VAN DER ZANDE, Zur Geschichtswissenschaft der Aufklärung: Johann Christoph Adelungs *Kulturgeschichte der Menschheit* (1782). TADEUSZ CEGIELSKI, Das Freimaurertum des 18. Jahrhunderts und die Quellen der romantischen Kunsttheorie. PETER HERSCHE, Jansenistische Sympathien in der deutschen Reichskirche im letzten Drittel des 18. Jahrhunderts. ROLF REICHARDT, Bastillen in Deutschland? Gesellschaftliche Aussenwirkungen der Französischen Revolution am Beispiel des Pariser Bastillesturms. RAINER VINKE, Johann Heinrich Jung-Stillings Reaktion auf die Französische Revolution. T. C. W. BLANNING, Die französischen Revolutionsarmeen in Deutschland: Der Feldzug von 1796. STIG FÖRSTER, Weltkrieg und Imperialismus: Der Einfluss der Revolutionskriege auf den Beginn der britischen Expansion in Indien 1798/99. LOTHAR GALL, Die Deutschen und Bismarck. ULRICH WENGENROTH, Hoffnungen auf Mitteleuropa: Absatzstrategien und Interessenpolitik der deutschen Schwerindustrie im Reichsgründungs Jahrzehnt. DAVID BLACKBOURN, Mittelstandspolitik im Deutschen Kaiserreich. STEVEN HOCHSTADT, Städtische Wanderungsbewegungen in Deutschland 1850–1914. RALPH MELVILLE, Der mährische Politiker Graf Egbert Belcredi (1816–1894) und die postfeudale Neuordnung Österreichs. BRIGITTE MAZOHL-WALLNIG, Imperatore o Patria? Loyalitätskonflikte lombardo-venetianischer Kämmerer. FRITZ FELLNER, Heinrich Friedjung—ein österreichischer Ahnherr der "Oral History." JOHN LESLIE, Österreich-Ungarn vor dem Kriegausbruch: Der Ballhausplatz in Wien im Juli 1914 aus der Sicht eines österreichisch-ungarischen Diplomaten. FERENC GLATZ, Das Deutschtum in Ungarn in der Zeit der industriellen Entwicklung. FRANCO VALSECCHI, Lombardo-Venetien und die Probleme der österreichischen Politik in Italien. MARINA CATTARUZZA, Nationalitätenkonflikte in Triest im Rahmen der Nationalitätenfrage in der Habsburgermonarchie 1850–1914. PETER HERTNER, Neuere Erklärungsansätze zur Geschichte der ersten Phase der Industrialisierung Italiens. FRANCIS L. CARSTEN, Die Quäker in Deutschland 1919–1924. FRITZ KALLENBERG, Die Staatsautorität der Republik: Der preussische Regierungspräsident, der Fürst von Hohenzollern und die Stadt Sigmaringen 1919–1933. MARTIN VOGT, Joseph Caillaux in der Aussenpolitik der Ära Stresemann. GYÖRGY RÁNKI, Kredit oder Markt? Zum Wandel der wirtschaftspolitischen Hegemonialbestrebungen der Grossmächte in Südosteuropa 1920–1931. GUSTAVO CORNI, Die Agrarpolitik des Faschismus: Ein Vergleich zwischen Deutschland und Italien. LOTHAR GRAF ZU DOHNA, Vom Kirchenkampf zum Widerstand: Probleme der Widerstandsforschung im Brennspiegel einer Fallstudie. WALTER F. PETERSON, Die deutschen politischen Emigranten in Frankreich 1933–1940: "Dieselben Debatten wie zu Hause?" JOHANNES HOUWINK TEN CATE, Das U-Boot als geistige Exportware: Das Ingenieurkantor voor Scheepvaart N.V. (1919–1957): Ein Beitrag zur

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REVIEWS OF BOOKS

TO THE EDITOR:

I appreciate John Frederick Schwaller's positive comments in his review of my biography of *Alonso de Zorita: Royal Judge and Christian Humanist, 1512–1585* [AHR, 94 (October 1989): 1207–08]. I am, however, concerned about what he considers "several disquieting features of the work."

He correctly notes that one of my footnotes (p. 332, n. 111) confuses Doña Beatriz de Estrada with Doña Beatriz de Andrada. But I do note on page 168 of the book that Beatriz de Andrada became the wife of Viceroy Luis de Velasco's brother after the death of her husband Juan Jaramillo.

Ideally, books should not contain errors. But they do, and one even finds them in book reviews. An example is Schwaller's statement that Zorita was "a young man in late fourteenth-century Spain." More to the point is my contention that Schwaller should have corrected my "misrepresentation of the facts" found on page 201.

I did accept royal commissioner Jerónimo Valderrama's assertions that three of Hernán Pérez de Bocanegra's sons married daughters of Francisco Vazquez de Coronado and Doña Beatriz, "one of five daughters of Alonso de Estrada, the treasurer and governor of the colony in the 1520s." I also assumed that Valderrama was correct in saying that Coronado was the viceroy's "primo."

Schwaller states, however, that the Pérez Bocanegra family was not related to Viceroy Velasco and that Doña Beatriz de Estrada was "Pérez Bocanegra's mother-in-law," which means that one or several of Valderrama's statements are untrue. Because Schwaller ap-

pears to know more than I or Valderrama about this matter—and more than Pedro de Castañeda and Herbert Eugene Bolton about Coronado's wife—he should have corrected my errors. Still, if François Chevalier is correct, there is a connection between the Bocanegra and the Coronado families. So, how badly have I erred in following Valderrama concerning Velasco's relationship with the Bocanegas, allegedly related to him by marriage ties with the Coronado family?

I am also bothered by Schwaller's statement that when "Vigil repeats on numerous occasions the complaint that officials, encomenderos, and other Spaniards loosed dogs on the Indians and hunted them down (pp. 91, 107–09, 113–14), . . . the reader never knows how much of this was real and how much was merely included in the complaints for shock value." But I do state (p. 91; p. 318, n. 24) that this charge was confirmed against Alonso López de Ayala, and the Council of the Indies ordered that the dogs be killed and forbade any Spaniard to raise or own dogs of this type in New Granada. Further, one of the great Indian haters of New Granada himself confirms the complaints. Pedro de Ursúa claimed that in the province of Guane "no hacía los castigos de indios que en este reyno se a acostumbrado facer como son de ahorcar y apperear e cortar narizes."

The use of dogs against Indians was widespread. To prove this, one only has to read some *residencias*, Gonzalo Fernández de Oviedo's *Historia*, Bernardino de Sahagún's *Florentine Codex*, or J. G. and J. J. Varner's *Dogs of the Conquest* (1983).

I am also puzzled by Schwaller's statement that I fail "to recognize the honorific *don* as a significant feature." He does note that I cite "scholarly criticism of those who used the honorific with Zorita (p. 10)," but he fails to say that I find Manuel Serrano y Sanz was wrong in claiming that Zorita was not a Don. More serious is Schwaller's claim that "[i]n this period those who deserved it always used it." The word "always" bothers me very much. Zorita, related on both the maternal and paternal sides to one of the fifteen great families of Castile, was a *don* but did not use the honorific. And, when Schwaller says that I remove the honorific "on occasion, from those who warranted it, leading to some confusion," it is his duty to give specific examples of this alleged error.

In summary, I believe that the review says more about Schwaller than about my methodology or interpretation of documents. I hasten to add that my pioneering work is not perfect. No study is. But it may be considered an ethical symbol (art related to life) and praise of one man's life. Whatever the book's faults, it gives meaning to Zorita, who admired common decency, who lived in the true and the right, who loved justice and wisdom and had charity.

RALPH H. VIGIL
University of Nebraska, Lincoln

John F. Schwaller does not wish to reply.

THE EDITOR

ARTICLES

TO THE EDITOR:

It is clear that President Eisenhower did not want the United States bled to death in a succession of small wars around the periphery of the Soviet Union and China; ergo, his willingness at times to consider the use of nuclear weapons. It is equally clear that—more than most people, starting with his first major foreign policy address in April 1953—he realized the unspeakable horror of nuclear war.

In "The Age of Vulnerability: Eisenhower and the National Insecurity State" (*AHR*, 94 [October 1989]), H. W. Brands comments on this dilemma, presents evidence—largely from minutes of National Security Council discussions—on Eisenhower's national security policy, and concludes that it failed.

Take the same evidence; rearrange it in rigorously chronological order; add crucially relevant evidence from other presidential sources—memoranda of conferences, diary entries, memoranda of phone conversations, personal letters; and include some mention of major crises that formed the context of decision making—for example, the Korean War in 1953, the Soviet threat to Britain and France in the Suez crisis of 1956, the threat to Lebanon in 1958, the threat to Berlin in 1958–1959—and one will, I believe, arrive at a far more plausible conclusion: that the Eisenhower national security policy in large measure succeeded.

In Ike's eight years as president, we had no bleeding peripheral wars—no Koreas, no Vietnams—and no Armageddon. And, incidentally, we did it all—the last time in memory—with some balanced budgets.

WILLIAM BRAGG EWALD, JR.
Greenwich, Connecticut

H. W. BRANDS REPLIES:

Because William Bragg Ewald does not dispute any points of fact raised in my article, I can only conclude

that our differences consist in our evaluations of the facts. I join Ewald in applauding Eisenhower for the mistakes he did not make and especially for his resistance to the temptation of war. Would that subsequent presidents had done as well.

Herein, I believe, lies the basis of the Eisenhower revival of the last ten years. After three decades featuring the Bay of Pigs, Vietnam, Watergate, inflation, and peacetime budget deficits unimaginable in any era before the 1980s, a presidential administration unmarked by comparable glaring failures appears benign and successful. Ewald's pride in his old boss is understandable.

Yet dodging disaster in the short term, however difficult for Eisenhower's successors, should not be the principal criterion for measuring presidential achievement. The actions of presidents, particularly in a field as fraught with danger and expense as nuclear weapons policy, have consequences that long outlive their authors. Eisenhower avoided war in Vietnam (although we could argue about his complicity-via-commitment in what came after), and he avoided Armageddon. But, by his decision to rely heavily on nuclear weapons, he encouraged the arms race that left the United States more vulnerable to destruction than when he entered office, more dependent on a military-scientific elite with interests distinct and often divergent from those of the country as a whole, and more susceptible to politically driven demands for higher defense budgets.

Ewald may judge this success. That is his privilege.

H. W. BRANDS
Texas A&M University

TO THE EDITOR:

I have just read the sharply honed, richly detailed essay of James T. Kloppenberg in the *AHR* of October 1989, titled "Objectivity and Historicism: A Century of American Historical Writing." I read Peter Novick's long and critical book, *That Noble Dream*, a few weeks ago. Both the book and the article may have profound effects on the profession and will be much discussed.

Now it happens that I followed (and still do) Carl Becker in his liberal skepticism and heard Charles Beard deliver his AHA Presidential Address in 1935, "That Noble Dream," and I have had certain experiences in the profession that make me want to comment. (See, for example, *Historical Study in the West* [New York, 1968]; "The Study of History in the United States," *AAUP Bulletin* 1964, pp. 232–40; *Proceedings of the XIV International Congress of Historical Sciences* [New York, 1976], 79–83.)

Both Novick and Kloppenberg recite so many "facts" that a reader might think the authors are trying to do history *wie es eigentlich gewesen*, which they are not. Nor can anyone fully. But if historians know their limitations, they can try to understand, and may—partially.

Still, Novick does not discuss some vital happenings that affected the "Noble Dream": the low income of

members of the profession and the joblessness of the young, especially during the 1930s. The then (1953–1963) executive secretary of the Association attempted to alleviate the situation by establishing the Job Register, a “fact” Novick does not mention. Nor does he or Kloppenberg discuss the 1954 year-long McCarthyite investigation of the Association by the Reese Committee of the U.S. Senate, which the executive secretary defeated by showing (a use of history) that the so-called incriminating statement, “the age of individualism is over and an age of cooperation is beginning,” was made not by a Marxist but by the Republican president, Herbert Hoover. This experience revealed that attempts to end the dream of free inquiry could be defeated. Nor do Novick and Kloppenberg mention the attempts of Jameson, Leland, and others, including the American who was elected president of the World Congress of Historians in San Francisco in 1975 to enlarge the “Dream” through international scholarly cooperation. And historians did increasingly eliminate discrimination by sex, nationality, race, and color—a desirable goal to which Novick and Kloppenberg should have given more attention.

Of course, for historians to understand anything as they ought, they must try to understand everything that has happened. Of course, they cannot. Nonetheless, they must try, even if the goal is unachievable, that is, if they really are historians seeking to recover the past in order to understand the present.

Among the critics of attempts to realize the Noble Dream during the mid-twentieth century were some professors at Wisconsin and North Carolina, at Washington and California, who thought profoundly, as a few do, who at times barked if they didn't bite. One of these was a well-to-do man who raised a storm because the Association attempted to charge a \$1.00 registration fee at the Annual Meeting. Another I remember was a brilliant, radical young scholar at Columbia whose criticism cut through verbiage and cant.

Attempts to enlarge the Noble Dream to include all varieties of history—public, academic, scientific, romantic, narrative, analytical—never ceased. It was also a “fact” that many historians cared little about their profession or about philosophical inquiries into the nature of knowledge. Still, in spite of them, inquiry proceeded.

It was not, of course, possible to achieve perfection. It was possible, whether at Wisconsin, “Cal” at Berkeley

or L.A., Harvard, Yale, Columbia, Chicago, The Johns Hopkins, or “po-dunk,” to understand human development psychologically, sociologically, economically, politically, anthropologically, or—to have fun and experience deep sadness along the way. It was a great illusion or delusion to believe that ultimate truth could be found, although deeper understanding was always possible and sometimes won.

The search went on, never stopped. Perhaps it is the journey, not the arrival, that counts, as Leonard Woolf remarked, and it is also the quality of the understanding, based on careful research and imaginative hypotheses, that counts. Historians could be and should be aware of their biases (this writer is a liberal Democrat and a skeptic), and state them openly, analytically test their hypotheses, seek insights, perhaps even seek social justice, as an American historian at Wisconsin advocated, and write factually accurate narratives, as Parkman and professors at Harvard and Columbia demonstrated.

As Kloppenberg remarks (*AHR*, 94, October 1989, 1029), “through a combination of imagination, technology, and diligence, historians have compiled hard data that now makes impossible some versions of the past that once passed as truth.” If going on to write history is then an “act of faith” (Beard), so be it. But some historians have offered us more: Charles Beard, Carl Becker, Natalie Zemon Davis, John King Fairbank, John Hope Franklin, Robert Palmer, James Harvey Robinson, Frederick Jackson Turner, Walter Prescott Webb, C. Vann Woodward, Gordon Wright, and . . . ?

BOYD C. SHAFER

Fayetteville, Arkansas

JAMES T. KLOPPENBERG REPLIES:

I appreciate Boyd Shafer's comments. Peter Novick's fine book will no doubt inspire many such reflections from historians who helped shape the AHA during recent decades. While reasonable people are bound to disagree about the success the AHA has enjoyed in its efforts to expand the range of historical inquiry and the diversity of those in the historical profession, I hope there can at least be consensus on the desirability of those goals.

JAMES T. KLOPPENBERG

Brandeis University

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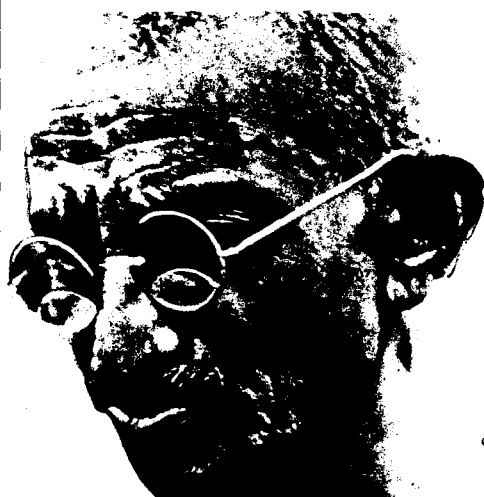
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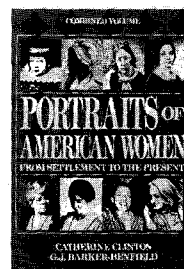
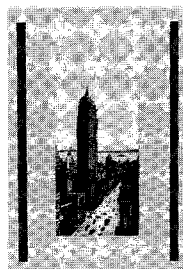
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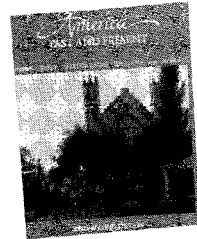
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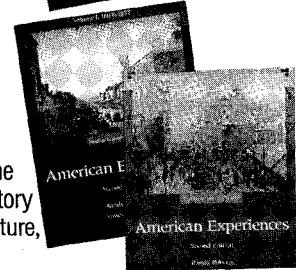
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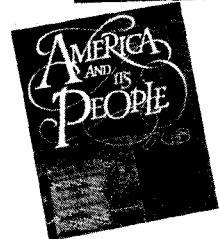
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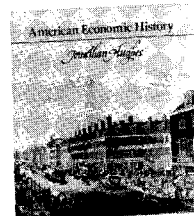
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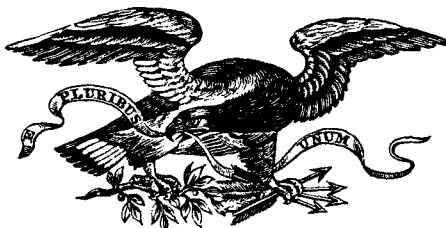
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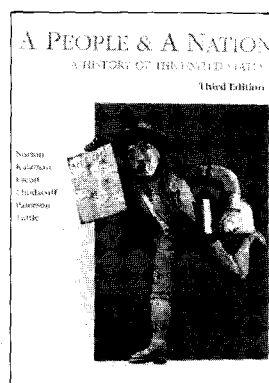
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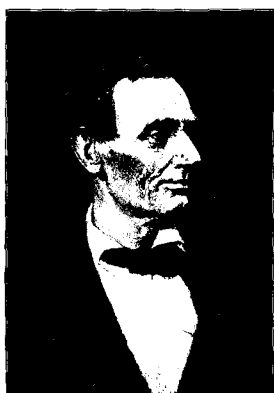
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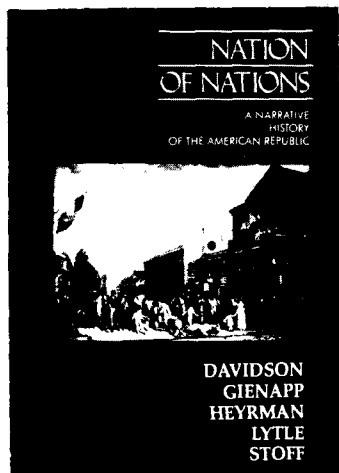
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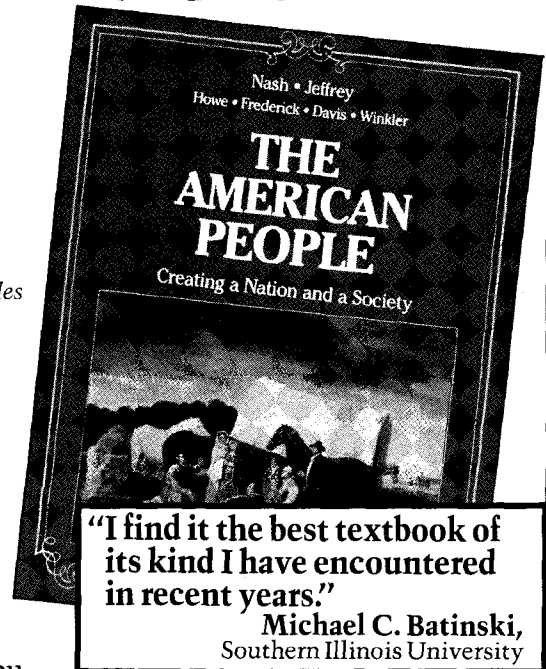
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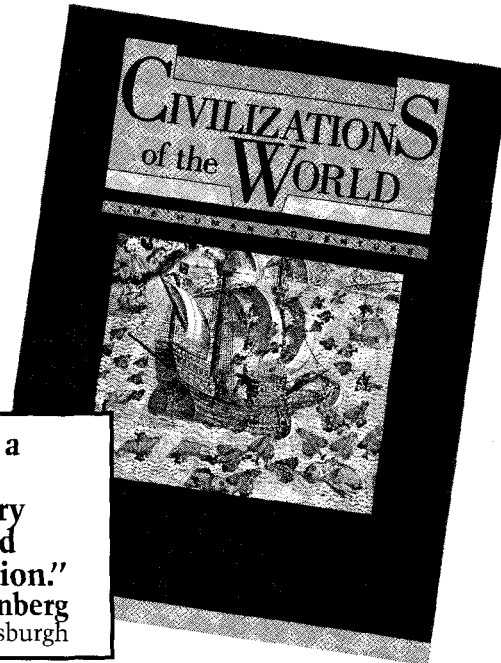
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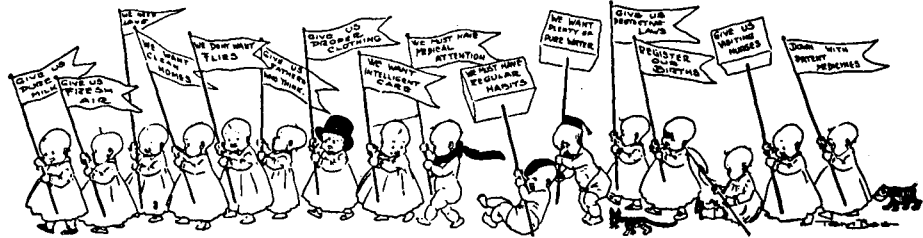
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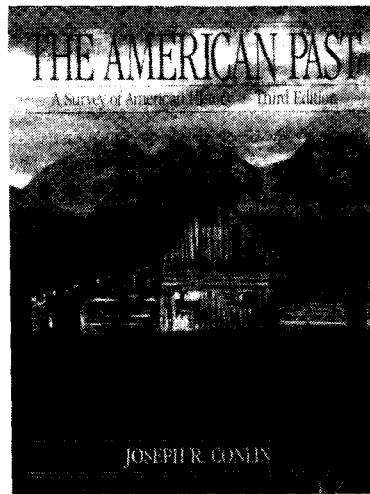
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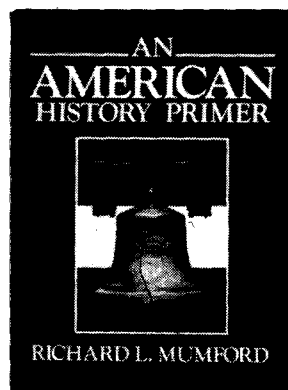
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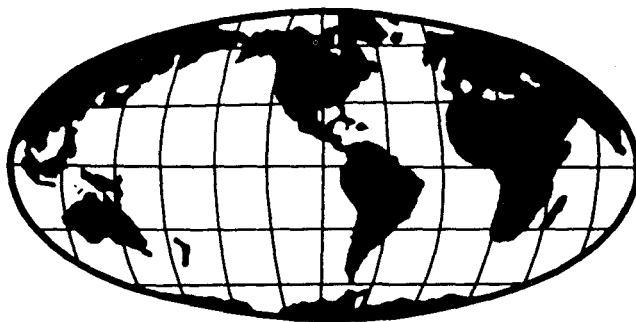
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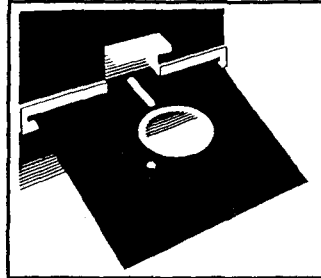
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